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CONDUCTED BY

SIR ROBERT KANE, LL.D. F.R.S. M.R.I.A. F.C.S.

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AND

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“Nec araneorum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes.” JUST. LIPS. *Polit. lib. i. cap. 1. Not.*

VOL. I.—FIFTH SERIES.
JANUARY—JUNE 1876.



LONDON.

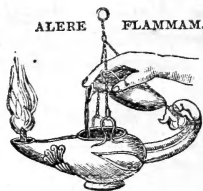
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“Meditationis est perscrutari occulta; contemplationis est admirari
perspicua Admiratio generat quæstionem, quæstio investigationem,
investigatio inventionem.”—*Hugo de S. Victore.*

—“Cur spirent venti, cur terra dehiscat,
Cur mare turgescat, pelago cur tantus amaror,
Cur caput obscura Phœbus ferrugine condât,
Quid toties diros cogat flagrare cometas;
Quid pariat nubes, veniant cur fulmina cœlo,
Quo micet igne Iris, superos quis conciat orbes
Tam vario motu.”

J. B. Pinelli ad Mazonium.



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- III. Illustrative of Dr. L. Bleekrode's Paper on Electromachines with Disks of Ebonite.
- IV. Illustrative of M. R. Pictet's Paper on the Application of the Mechanical Theory of Heat to the Study of Volatile Liquids.

ERRATA.

SERIES 4, VOLUME 50.

Page 405, line 27, *for* $T = 34.8636$ sec., *read* $T = 34.836$ sec. for the second experiment of Kohlrausch,

— 526, last line, *for* $\frac{1}{10n-}$ *read* $\frac{1}{10n-7}$

SERIES 5, VOLUME I.

Page 348, line 10 from bottom, *for* diameter *read* thickness.

— 349, line 9 from bottom, *for* active *read* inactive.

— 370, line 4 from bottom, *for* $\frac{dU}{dv}$ *read* $-\frac{dU}{dv}$

— — — 3 — *for* $\frac{dU'}{dv}$ *read* $-\frac{dU'}{dv}$

THE
LONDON, EDINBURGH, AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[FIFTH SERIES.]

JANUARY 1876.

I. *On the First Principles of Chemistry.* By EDMUND J. MILLS, D.Sc., F.R.S., Young Professor of Technical Chemistry in the Andersonian College, Glasgow*.

CONTENTS.

(1) A property of First Principles.—(2) Special difficulties of the investigation.—(3) Need of it.—(4) The first principle of science is motion—(5) of chemistry, action.—(6) Such action is determinate, continuous, proportional to mass, and occurs between wholes; it is not, therefore, expressible by the ordinary symbols.—(7) Experiments of Gladstone, Harcourt & Esson, Guldberg & Waage; those of Bunsen and Debus are consistent with continuity. Chemical action cannot be regarded as finite.—(8) Acting substance is directed motion; homogeneous, not partite. Digression on isomerism.—(9) Consequences traced: vices of current speculation: diffuse and concrete motion.—(10) Symbols of substance, operation, equivalence. "Valencies" cannot form an arithmetical series. Equivalent defined.—(11) Table of theses and anti-theses.—(12) Principles on which to found a decision.

(1) **F**IRST principles are of all portions of a science the most difficult to grasp. The explanation of this fact lies in the law, first pointed out by Professor Ferrier (*Institutes*, p. 12), that what is first in the order of nature is last in the order of knowledge. To that subtle and accomplished philosopher (now, alas! no longer with us) the pure reason of mankind stands in eternal debt, and not least for the power and beauty of his exposition of this law of scientific progress. For, if it be progress to attain first principles, then "all science can advance only by going, in a manner, backwards, or rather by *coming round*;" and "the apotheosis and final triumph of

* Communicated by the Author.

Phil. Mag. S. 5. Vol. 1. No. 1. Jan. 1876.

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human reason will be, when, having traversed the whole cycle of thought, she returns—enriched only with a deeper insight and a clearer consciousness—to be merged in the glorious innocence of her primitive and inspired *incunabula*.” The same thought, in its moral aspect, constitutes the lesson of the Prodigal Son*. The advancement of science, the excursus of the soul, are popularly represented as straight lines; on the contrary, they are returning curves. “The primitive articles of all thought, the seminal principles of all reason, the necessary constituents of all knowledge, the keys of all truth, lie at first buried under our very feet; but, as yet, we are not privileged to find them. We must first circumnavigate the globe; the whole world of speculation must be traversed by our weary feet. Hence every step forward carries us only further and further from the mark. Ere long the elements of truth—all that we are indistinctly looking for—lie in the far distant rear, while we vainly think that we behold them glimmering on the horizon in our front. We have left them behind us, though we knew it not—like decaying camp-fires, like deserted household gods.” “The intellectual, like the physical world, is *a round*; and at the moment when the wanderer imagines himself furthest from the house of humanity, he will find himself at home. He has revolved to the spot of his nativity. He is again surrounded by the old familiar things. But familiarity has been converted into insight; the toils of speculation have made him strong, and the results of speculation have made him wise” † (*ibid.* pp. 10, 11).

(2) The difficulty of the investigation, however, is capable of large increase from special conditions. Consider the case of chemistry. Detail has been added to detail, concrete result to concrete result, during an enormous interval of time; and the pure jewels of the science lie deep under a roadway, which has been hardened by the traffic and elevated by the rubbish of at least two thousand years. The ideal reformer who would disinter them must consecrate himself to the task, and live to universals; he must wrench himself from contemporary prejudices and associations, and, regarding each fact and theory in

* Trench (Notes on the Parables, 1870, pp. 387, 388) supposes, in accordance with the common interpretation, that the “younger son” is principally intended to illustrate the *sin* of desiring spiritual independence. The whole meaning turns upon the words *Εἰς ἑαυτὸν δὲ ἐλθὼν* (But when he came to himself), the attainment of ideal independence by way of experiment being the very point of the story.

† Compare also Mill on Utilitarianism, p. 2 (1863):—“The truths which are ultimately accepted as the first principles of a science are really the last results of metaphysical analysis, practised on the elementary notions with which the science is conversant.”

the light of this pursuit alone, deliberately put aside all judgments which are based upon dogma or authority. He must have an infinite capability of doubting. For his is no search for truth—of which all men chatter, but which none are admitted to possess; his is a reasoned labour; and the issue, of which he is careless, must in the main be wise. Such being the case, can we wonder that no clear statement of the first principles of chemistry has ever yet been made, and that even the very definition of the science is either unknown or obscure? Is it remarkable that, in the hundreds of chemical manuals that have been written, little or nothing is said of those secret springs that feed the literary stream, or of the dews that fertilize the less familiar province of experimental research?

(3) Yet this investigation is much needed. I beg any dispassionate observer to survey the field of modern chemistry, and ask himself whether its condition has been satisfactory of late years. For my part, I came neutral to the task, with no tradition to support and with no interest but to inquire. Could I possibly justify the prevalent spirit in the science? Was the method of its theories reasonable? Did its leading doctrines harmonize with those of other sciences, and tend to ennoble man? And what was the cause that, even in popular opinion, it appeared unfruitful of discovery, and languishing rather than alive? The reader already knows the general tenor of the answers. Strength and light and vigour can only come from some fundamental reform, sufficient to alter the entire aspect in which chemistry is contemplated, and the entire method whereby chemical problems are solved. This reform can be reached, and only reached, by turning back to first principles.

(4) Where, then, are we to commence? The first principle of all science is motion. Every event of which we are conscious proves, on analysis, to be motion of some kind; and matter is not distinguishable from motion except as more or less determinate motion. Strictly speaking, "the motion of a thing" is a tautological expression, unless we mean by "a thing" another kind of motion, when the phrase becomes equivalent to "compounded motion." From the rigour of Hobbes's logic* there is no escape; indeed the almost unanimous voice of philosophy, from ancient to modern times, has asserted the universal prevalence of motion, the absence of all real rest. This grand idea has changed the character of human action wherever its mission has been accepted and its reality has been felt. Of late years it has, in some derived form, completely renovated and restored whole sciences. What is the life of

* *Humane Nature*, second edition, pp. 9-15.

modern geology or mathematics, but continuity ; or of biology, but evolution ? The greatest generalization of the practical consciousness is worthy to become our chief criterion and lay our fresh foundation.

(5) We have then to select those derived forms of the idea of motion which, lying nearest to it, are yet within the province of chemistry. One of these must necessarily be *action*, because the chief business of chemistry is a kind of work. Chemical substances are valued, not for what they are conceived as *being*, but as *doing* ; and the first question we ask about a body is, What is its function ? alcoholic, saline, ketonic, zincous, chlorous ? in other words, How does it behave with this, that, or the other *reagent* ? And it is made, bought, and sold for its *use*.

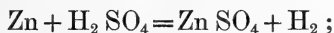
One objection that might be made here ought perhaps to be answered at once. It may be said that substances have a being in themselves—namely, a “constitution,” as it is technically termed—that their constitution consists in the arrangement of atoms, of which they are in reality made. Whether this is the case or not, shall be in due course examined. I content myself with remarking that constitution has been, and always is, considered of very small importance in comparison with action,—the two standing to each other in the relation of speculation and experiment. A moment’s consideration of particular cases will show this. Indigo, sugar, salt, and alcohol were extremely valuable before any thing whatever was asserted of their constitution—valuable then as now, for the *employments* to which they could be put, the producer prudently regarding their constitution with a wise indifference. Hence, then, we are on sure ground in adopting action as a first principle, whether we should have, or not have, to accept “being” or “constitution” at a later period.

(6) Chemical action, however, is not indeterminate. If it were so, all possible reactions might occur at one time, and all reactions would be possible. Isomerism could no longer arise. The two mononitrotoluols, for example, that are formed during the nitration of toluol could possibly be converted into each other, and would yield a confused or mean nitrotoluol ; nay, rather, any nitric solution of toluol would be as definite a chemical substance as we now consider nitrotoluol itself. Thus the old theological guess, *Deus est actus purus*, cannot be transcribed for chemistry ; in this science, action is determinate, conditioned, and a process.

The most important character of chemical action is *continuity*, which, as has been already stated, is an immediate derivative of the idea of motion. When two or more substances

are placed together in such conditions as to react chemically, various kinds of work are performed, and other substances are found to be present. Direct contact is not always necessary ; it may suffice that a shock or tremor traverse the intervening or diachemic medium, in order to produce the desired result. This is well seen in the experiments of Abel and others, where nitroglycerine, gunpowder, and the like have been fired without such contact, by the effect of distant explosions of particular quality.

Continuity is, moreover, observed in the relation of the masses of the acting substances. According to the evidence we possess (and some of it is of the highest order of accuracy), *no matter what may be the masses of the substances reacting, the entire mass of each takes part in the process*. If we immerse an ounce of zinc in water, and add to it a quantity of hydric sulphate, the whole of this reagent acts, whether it weigh an ounce, a pound, or a hundredweight ; and it is known by experiment that if water, zinc, and hydric sulphate be placed in contact, the rate, amount, and quality of the action all depend on a cosmic law—the masses of the acting system—and vary as these vary*. Of this rich and indefinitely great variety of process, the Daltonian symbols show us but a poor and single point,—



all the hydric sulphate that is not required to make zinc sulphate, though undeniably sharing chemically in the process, is omitted from the expression ; and it may be added that the equation itself, one of the most frequently written in books and memoirs, has never yet been realized in the history of chemistry. May I suggest to those students who are seeking subjects for investigation, how much more profitably their time would be employed in examining the commonest definite equations, than in such enterprises as trying to find out by experiment what is the position of the third bromine atom in tribromobenzol ?

(7) The mind of Berthollet was clearly impressed with the necessity of reconciling the laws of chemistry with those of astronomy. As he contemplated the evening sky and watched the marshalling of its glorious host, no thought of atoms rose (as in Dalton) with those distant clusters ; but the conviction that the earth's laws are not discrete from those of heaven, and that the principle of celestial attraction must be identical with, or animate, the chemical process. The law of chemical mass,

* For the demonstration in the case of gases, see Bunsen's 'Gasometry,' p. 254 (English edition).

though thus foreseen by Berthollet, was not placed by him on a complete experimental basis ; and the idea that both he and Bergman perhaps possessed, that each substance has a mass of its own, has probably been investigated but by a single chemist. Later researches have, however, placed the law in a clearer light. Gladstone, for example, in 1855*, showed that, in a certain group of reactions, "There is nowhere any sudden increase. . . . If the partition of the bases and acids in the mixture really take place at first in atomic proportions, it is evident that, being at full liberty to act and react, the salts arrange themselves according to their respective mass, without reference to their respective atomic weights"†.

It is, however, more especially to Harcourt & Esson and Guldberg & Waage that we are indebted for the investigation of this subject. Thus, for example, the former chemists examined the deportment of mixtures of potassic permanganate, hydric oxalate, hydric sulphate, and manganous sulphate in presence of a quantity of water which may be regarded as constant, the effect particularly traced being the oxidation of the oxalate by the permanganate present. The authors invariably regard the entire weight of a reagent present as active ; and they prove that the amount of oxidation that takes place is directly proportional to the total amount of permanganate remaining in solution at a given time. In a second inquiry into the reduction of hydric or a similar peroxide by hydric iodide in an aqueous solution of varied content, they show‡ that "whether the solution contains in each cubic centim. 746 millionths of a gramme of hydric sulphate, or 150 times that quantity, 604 millionths of a gramme of potassic iodide or 9 times that quantity, or whether hydric chloride or hydrosodic carbonate be substituted for hydric sulphate, whether the temperature be 0° or 50° C., and whether the portion of change require for its accomplishment intervals of one or two minutes, or intervals of half an hour or an hour, this reaction still conforms to the law that the amount of change is at each moment proportional to the total amount of changing substance." The processes above referred to, as well as others confirmatory of them, are represented by Esson as continuous lines ; and where then the case is not too complicated to treat, the law of action is figured graphically as a hyperbola or, ordinarily, a logarithmic curve.

* A reduction of all the most important of Gladstone's numerical results has been given by myself in this Journal [IV.], October 1874, where they are shown to agree with the formulæ of certain logarithmic curves, the use of which was first introduced into chemistry by Esson.

† Phil. Trans. 1855, p. 189.

‡ Phil. Trans. 1866, pp. 127, 128.

Similar results were arrived at by Guldberg & Waage*. They found that the force which produces the formation of A' and B' increases proportionally to the coefficient of affinity (k) for the reaction $A + B = A' + B'$, and is proportional to the product of the active masses (p, q) of the two substances A and B: they equate the force to kpq , and, on this basis, find themselves able to represent the results of a long and detailed experimental investigation into the measurement of chemical attraction. They have also shown that the results of the researches of Berthelot, Scheerer and Debus are capable of representation on their principles. But their examination of Debus's research on the fractional precipitation of mixed baric and calcic chlorides by sodic carbonate deserves especial notice, because the numbers obtained by that chemist had long been supposed to support the ordinary view that chemical action is characterized by discontinuity. No real break, however, occurs. Of his three investigations, they take that † in which, according to its author, the experimental data are least open to objection. Here the only variable is calcic chloride (p), and the precipitate is baric carbonate (x) (here represented as sulphate); and the weight of the latter is given in terms of the former by the equation

$$x = \frac{0.389 - 0.035p}{1 - 0.080p},$$

an expression in which no discontinuity is involved. It is, indeed, remarked by Debus himself, that what he regarded as sudden changes are very similar to the gradually increased distention and ultimate fracture of a string stretched by a weight.

Bunsen's well-known experiments on the explosion of mixtures of certain gases also need consideration here, for the reason just referred to. The following Table ‡ is a specimen of the results obtained:—

Comp. mixture.			Consumed by detonation.		Ratio $\frac{\text{CO}}{\text{H}}$.
CO	H	O	CO	H	
72.57	18.29	9.14	12.18	6.10	2 : 1
59.93	26.71	13.36	13.06	13.66	1 : 1
36.70	42.17	21.13	10.79	31.47	1 : 3
40.12	47.15	12.73	4.97	20.49	1 : 4

Now, suppose that, instead of causing the above four mixtures to explode, Bunsen had been able to contrive some means

* *Études sur les Affinités Chimiques*, Christiania, 1867.

† *Ann. Chem. Pharm.* vol. lxxxviii. p. 238.

‡ Watts's 'Dictionary of Chemistry,' vol. i. p. 860.

allowing of their gradual reaction (such, for example, as is used in a gas-battery), and had examined the composition of the gas from time to time. Then doubtless it would have been found that the peculiar relation he observed is reached continuously, and is merely a maximum value of one of the functions in the equation to the reaction; and this would have been noticed in each of the four cases. Thus each abrupt relation would appear as but one of a long unbroken series. The experiments, however, were not so designed as to show this, or give any chance of showing this; on the contrary, they were explosions—broken, abrupt, discontinuous—of set purpose. To deduce discontinuity where we have expressly introduced it, is to beg the entire question. But even so, the evidence of a break is fallacious. We might as reasonably infer, from the striking of a clock at regular intervals, that it had not been previously going, though in fact the entire phenomenon is ultimately based on the gradual uncoiling of a spring. In like manner all the “definite and multiple proportions” of chemistry depend upon, and are evidence of, some action exerted continuously.

The process of exhausting the chemical energy of a substance, as represented mathematically in logarithmic equations, requires an indefinitely great period of time for its accomplishment. Hence we can understand how chemical action is possible. *It can begin because it never has ended.* During any period we choose to assign, every substance retains a minute but real reserve of unexhausted energy, sufficient to meet or to induce some further attack. For it is as experimentally certain that the generated substances take part in a reaction as it is that the generators share therein. When, therefore, a body is enclosed in some vessel and set aside, it must not be regarded as inactive; it may quiver, but will not expire. Thus has the old adage, *corpora non agunt nisi sint soluta*, been deposed by modern research; and “*affinitates quiescentes*” have regained more than their ancient importance.

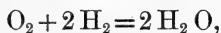
It appears, then, that chemical action is a process at once definite and continuous. Those positions in its course which Dalton termed “definite proportions” are fitly conceived as maxima or minima on curved lines.

(8) In this place we may consider the question, What are the substances that act? As has been stated in (4), matter is directed motion; and, after considerable reflection, I feel strongly convinced that the definition is both sound and adequate. Hence we conceive that matter cannot be made up of discrete parts, whether we term these atoms, monads, particles, radicals, or centres of force. This inference, which is ulti-

mately based on Hobbes's analysis of sensation, and therefore upon experience, appears to me unimpeachable; nor has any one yet been able to discover, either by accident or the most ingenious design, a single atom or ultimate point of any kind. Parts do not exist until we make them, and then they are new wholes. If, for example, I cut a piece of paper into two parts, it cannot be correct to say that those parts existed in the paper before; and they are now no longer parts, but two new whole pieces of paper. We may proceed a step further. Water is usually said to consist of hydrogen and oxygen. How can it do so when they are gaseous bodies, and it a liquid, at the ordinary temperature? The answer is, Because you can obtain hydrogen and oxygen from it. We might as reasonably affirm that a chrysalis is a butterfly. Having added to water an enormous amount of energy, the result has been two gases; resume the energy, and water is reproduced.

$$\text{Water} + \text{energy} = \text{hydrogen} + \text{oxygen}.$$

The common statement omits "energy" and is therefore an erroneous equation; for it leaves the student open to infer, as many young students do, that water is decomposable without any exertion whatever. The phenomena are, in fact, quite consistent with hydrogen, oxygen, and water being either of them compound or simple. Thus, instead of writing



we might with equal exactness represent the experiments by the equation

$$\begin{array}{rcccl} (p+x) & + & (p-x) & = & 2p, \\ 32 & + & 4 & & 36 \end{array}$$

which exhibits water as a simple body, just intermediate between hydrogen and oxygen. I examined this same question some years ago, when investigating a kindred topic—the ideas relating to chemical substance. In a memoir upon this subject, I pointed out that the principle of classification, as applied to chemical substance, at once shows that we have been gradually conceiving it as *homogeneous*, and chemical substance is analytically defined as *homogeneous substance*. "Each element is, as has been stated, homogeneous; that is, the whole list of elementic discriminants fails to show that it consists of more than one thing, or that it can be made by putting two or more things together. Each amine is homogeneous; because the varied application of aminic discriminants, such as potassic hydrate, hydric chloride, platinic chloride, reveals one thing only"*.

* Phil. Mag. [IV.] vol. xl. p. 261.

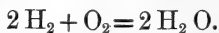
I cannot dismiss the question of chemical substances without referring for a moment to those that are isomeric. The general phenomena of isomerism are conceived to point irresistibly to an atomic theory of some kind. As I have discussed this topic elsewhere*, and shown that all the *experimental* evidence on this subject leads up to continuity, I will not again refer to it, but merely adduce the following simple illustration of my view. Imagine a number of insulated Leyden jars, alike in weight, volume, and every other particular of construction. Let each of these be charged with a different amount of electricity; and let the electricity be positive in some jars, in others negative. Now let the same piece of charged gold-leaf be caused to approach the knob of each jar in succession, its initial state being restored before each approach: in no two cases will the attraction (or repulsion) be the same in amount. This is exactly the position of the phenomenon of isomerism. All we know of it is that certain bodies, having equal (gaseous) volumes for equal weights, behave differently when a standard energy is applied to them. To measure, both in quality and quantity, the energy associated with each isomeric substance, is to give a complete account of the phenomenon of isomerism. Into this account the atomic element does not enter.

Thus, then, whether we consider the act of mechanical division or of chemical decomposition, or the fundamental idea educible from the classification of chemical substances, or what is the most real meaning of our sensations, the answer is practically a single one. The substances that take part in chemical actions have no parts or radicals; they are *homogeneous*, consisting of directed motion.

Energy is only known to us as diffuse (non-isolable) motion; matter, as concrete (isolable) motion. Whether the one kind can be transformed into the other, and what, in case such a change can be effected, is the nature of the intermediate substances, are questions which I can only raise, and not solve. Until periodic verifications are made of the quantities in which standard elements combine, we have not even approached the threshold of an experimental answer. No one, for example, can assert that argentic chloride has the same composition now that it had 100 years ago. So uncertain is the axiom that no matter can ever be lost.

(9) Some consequences of this result must now be traced.

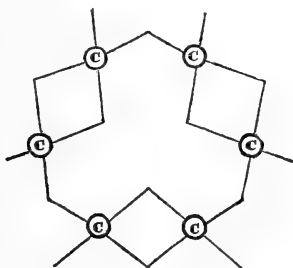
(a) The vices and virtues of the common chemical equations are typically depicted in the well-known expression



* Laboratory, 1867, p. 54.

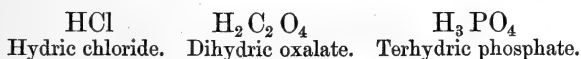
But this equation is a *suppressio veri*, by omitting “+ energy” from its right-hand side; and a *suggestio falsi*, by proposing to consider any bulk of water as made up of at least three parts. We must therefore abandon the common chemical equations.

(β) On the ordinary supposition, bodies are supposed not only to have parts, but to have such parts disposed in a certain way; this is their *constitution*. The constitution of benzol, C_6H_6 , is, for example*,



But it has been shown that quantity does not consist of parts at all, however arranged. The theory of “constitution” is therefore shattered by the same stroke that smote its parent the “equation.”

(γ) According to the Berzelian nomenclature (revived in recent times, and by far the most scientific that has yet been devised for chemistry), formulæ are represented by dual names which correspond to the specific and generic names of biology. Thus we have



similar to *Pinus sylvestris*, *Geranium robertianum*, &c. These names, therefore, suggest what the formulæ suggest; and, with the formulæ, they must retire. The less grammatical names, such as sodium chloride, calcium nitrate, are clearly involved in a similar fortune.

Here I may perhaps be encountered by the question, What is to be the instrument of discovery and reasoning if the atomic theory be discarded? This inquiry is sometimes so confidently made, that it seems to be deemed unamenable to a reply. But the answer is obvious, and it is furnished by history. Chemistry existed before Dalton. The discoverers of the cause of the increase of weight of metals by burning, of the chemical

* Kekulé, *Lehrbuch*, vol. ii. p. 496.

nature of respiration, of the composition of water, of the first accurate quantitative methods in analysis, were none of them atomic theorists. Can we say that, aided by the atomic theory, we have equalled or surpassed those great achievements? And if the atomic theory is indeed the legitimate parent of the interminable horde of derivatives, isomers, polymers, and the like, which swarm across the science, why has it done nothing, or so little, to train and educate them? Or dare we venture to surmise that there must be something inherent in the very nature of that theory which accounts for this pauper progeny—which explains why so many substances are made and then set adrift in literature unmeasured, unclothed, and requiring to be investigated all over again. The object of chemistry has become an art of breeding. May it not be possible that many minds, habituated to the ease and fancies of atomic speculation, have lost the rigour and hardness required for the exactest research? The examination of natural phenomena is best effected with the minimum of assumption of any kind; and instead of demanding a theory, it would be better to ask how we could dispense with one. It is juster and wiser to adhere to facts than attempt to transcend them.

(10) Chemistry is extremely deficient in symbolic expressions for its facts, and, except in Brodie's partly published calculus, has none to which mathematically deductive processes can to any extent be applied. Provided that any equation does not violate the law of even numbers, and will add correctly, we may write out any grouping of symbols we please, and the prospect *may* hold; but we cannot say that it *must* hold. Contrast the enormous resources which are at the disposal of the mathematician when he wishes to represent the position of a point, and the light and fulness of his representation, with the pitiful poverty of "modern chemistry" (as it is triumphantly termed), whose equations only tell you in substance that two and two make four. Assuredly it is time that chemical investigators approached in earnest the subject of a new symbolic system, and sought for data to lay it upon a broad foundation. At present such important processes as continuous etherification and continuous saponification are utterly unable to be suitably expressed. Some suggestions towards the desired result may be stated as follows.

(α) From what has been several times repeated in this memoir, it will be apparent that *every symbol, whether of chemical substance or of chemical operation, must represent a whole.*

Mathematical methods involve no discontinuity, and may therefore be applied to such symbols. Thus instead of writing $2\text{H}_2 + \text{O}_2 = 2\text{H}_2\text{O}$, some equation of the form $2x + y = z$ would

be legitimate. Here no assertion is made that water is a compound body.

(β) The use of such symbols will depend upon the object in view. One instance of their application has been given in (7), where the effect of one variable (calcic chloride) on another variable (baric sulphate) is thereby expressed. The logarithmic and hyperbolic curves of Èsson, in which are shown the relations of changing substance to change produced, also make use of such symbols. These are representations of the chemical process.

But we require to use symbols for other purposes. Supposing that, in Debus's investigation, baric iodide had been used instead of baric chloride, what weight of the former would have done the same work as the latter? The answer to this question gives us the real or *dynamic equivalent* of baric iodide in terms of baric chloride. In other words, if i stand for baric iodide and c for baric chloride, it is a solution of the equation

$$i = fc.$$

It was after such numbers that Bergman sought in vain; but he was the first who clearly saw their immense importance. In memory of him we may term them Bergmannics. For the few that are known I refer to the Proceedings of the Royal Society, vol. xviii. p. 348, and this Journal (IV.), vol. xlv. p. 506*. Thus, if N stand for thalious, argentic, or plumbic nitrate reduced to the unit (NO_3), and P stand for potassic nitrate, the bergmannic is

$$N = \frac{8}{5} P;$$

if S stand for sodic nitrate,

$$S = 1 P.$$

Diffusion bergmannics were obtained by Graham (Phil. Trans. 1850, p. 46), who proved that they are not by nature atomic, but either equal weights or multiples of equal weights. Such questions are of the highest importance. The manufacturer, for example, who is required by alteration of prices or other economical reasons to substitute one substance for another, ought to have such numbers ready to hand. Should the agriculturist desire to use potassic instead of sodic nitrate on a grass crop, he ought to know what weight of grass will be produced by one as compared with the other. The physician should know what weights of quinine, cinchonine, and strychnine, &c. do the unit of work. All this, and more, is not only attainable but near.

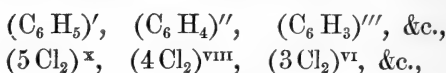
* See also Chiżyński, *Ann. Chem. Pharm. Suppl.* vol. iv. p. 226.

Having harvested such dynamic units, and classified them upon a common scale, we should certainly see a new calculus and a new nomenclature. Instead of Daltonian atomic weights, we should have the realities which I have termed bergmannics; and chemical substances, instead of being named dually, on the current system, would be called by their bergmannic symbol. Chemistry is now in the same position as we should be, if, with the sovereign, the shilling, the penny, &c. in circulation, we were unable to express the value of one in terms of the others. For the common "equivalents" are not, so far as we know, equivalents at all. They are defined, for instance, as the weights of Li, Na, Ag, &c. that will displace H in the expression HCl. We might as well say that the value of the loads in a railway truck are always equivalent, whether the load consists of air, of hay, or bullion. It is sufficiently obvious that the real equivalent of a body is that which performs the unit of work, however that unit may come to be defined*.

The law of decrease of chemical energy throws considerable light upon this question of equivalence. Let, for example, the "successive steps" in the chlorination of benzol be one and the same kind of operation throughout; let $+B$ represent that chemical property of benzol (it is enough to assume *one*) which is reduced by chlorination, and $-C$ that chemical property of chlorine which accumulates on the benzol by chlorination; also let β be a factor less than 1, γ a factor greater than 1. The law in question leads, at its simplest, to the equation

$$y = B\beta^x - C\gamma^x,$$

y being the sum of the aforesaid properties after a stage x of chlorination. Thus the residues $B\beta^x$ and $C\gamma^x$ form geometric series, the actual "valencies" of which decrease therefore by a common factor, not alter by a uniform difference. In other words, the arithmetical series of valencies,



is inconsistent with the law. This result affects the entire doctrine of substitution. It also justifies Erlenmeyer's remark, that there cannot really be any such thing as a polybasic acid.

(11) The following summary will serve to exhibit in a clear light the difference between the chemical principles I advocate

* One instance of such a definition might be,—*The equivalent of a substance is that weight of it which, in a tube of unit capacity, liberates the unit weight of iodine in the unit of time.*

(the *Theses*) and those which are more or less ostensibly current (the *Antitheses*).

Theses.

- i. The first principle of science is motion.
- ii. The first principle of chemistry is action.
- iii. Such action is determinate, continuous, and takes place between wholes; it cannot be regarded as finite.
- iv. Acting substance is homogeneous, and consists of directed motion.
- v. Symbols of substances should be homogeneous, and represent wholes; equations of action must be continuous.
- vi. α . The equivalent ("bergmannic") of a substance is that weight of it which performs the unit of work.
 β . If there be "valencies," their series must advance by a common factor.

Antitheses.

- i. The first principle of science is matter.
- ii. The first principle of chemistry is neutrality, indifference, or saturation.
- iii. Chemical action is indeterminate or partially determinate; it occurs in successive stages, between very small and indivisible parts; and it is completed almost instantly.
- iv. Acting substance consists of moving or stationary atoms.
- v. The symbols of most substances should represent composition; the equations of action are identical with those which show the mere final distribution of weight.
- vi. α . The equivalent of a substance is that weight of it which takes the place of a unit weight of hydrogen.
 β . There are "valencies," and their series proceed by a common difference.

(12) What, then, is to be the practical result of this discussion? Some interest may perhaps attach to the mere exhibition of a chemical system so different from that which now prevails, and sundered therefrom by so broad an interval; but the result would then hang lifeless, with no more purpose than a picture on a wall. Neither has it been my object so to convince the reader as to awaken in him some sudden enthusiasm for a new, or rather for the old, cause. I call upon him to consider for himself which system, the atomic or dynamic, he deliberately prefers; to ask himself which of these, either in its largest generalizations or minutest details, best corresponds to his reasoned views of nature; and, finally, to decide (for it is worth deciding) to which he could assign those elements of rectitude, of justice, and of well-being which, as a member of a social order, he values most. For, in the long run, no part

of ourself is dissociated from another part ; and the principles which underlie chemical work this hour, become in the next the springs of moral action. Here, then, is neither mist nor vagueness ; these tests are decisive and at hand. All real philosophy is human ; its foundations lie deep in the granite of every day experience ; and the superstructure is lucid as the noon. In that keen light I place the issue.

II. *On the Microscopic Characters of some Peculiar Forms of Coke.* By E. TULLY NEWTON, Assistant Naturalist H. M. Geological Survey*.

[Plate I.]

IN the extensive collection of fuel substances formed by Dr. Percy, F.R.S., there are certain specimens of Coke presenting very peculiar forms. The Doctor kindly placed these specimens in my hands that I might have the opportunity of examining them microscopically, and also suggested the desirability of publishing a short account of my observations.

The specimens are of two kinds, both obtained from a coke-oven, and are so unlike each other that there can be no doubt that they have been formed in an entirely different manner. One of them has a silvery metallic lustre, and is usually attached to some object, either as a flat expansion, or in masses of feather-like branches (Plate I. figs. 1 & 2), having much the appearance of minute stalactites. The surface, which is sometimes smooth, mostly appears to consist of a number of minute globules or bubbles closely packed together. When any considerable thickness is formed it becomes very hard and dense. Specimens of this kind of coke may be seen in the Museum of Practical Geology, Jermyn Street ; and perhaps the most remarkable example is that in which some plaited cane and straw, and also some clay tobacco-pipes, are coated over with the most beautiful feathery stalactitic masses of this peculiar coke.

The bubble-like appearance of some specimens might lead one to suppose that they were formed by the bubbling of the semifluid hydrocarbons, in a manner similar to that which may be often seen when ordinary coal is burnt in a grate ; but the fact that objects, such as the tobacco-pipes just mentioned, become coated with this coke, shows conclusively that this cannot be the case. And, again, the bubbling process

* Communicated by the Author.

produces light vesicular masses, while the kind of which we speak is very dense. It seems most probable that, when the gaseous hydrocarbons evolved in the process of distillation become more highly heated, they are decomposed and a deposition of carbon takes place, as described by Dr. Percy in his volume 'On Fuel,' 1875, p. 419. The Chinese give some of their common earthenware a coating of carbon of this kind, which gives it a metallic lustre. Mr. C. Tookey, formerly of the Japan Mint, tells me that the Japanese use, for the roofs of their houses, tiles which have been coated with carbon in a manner similar to this.

The second kind is that which is commonly known as hair-like coke, and is interesting inasmuch as it presents, when examined with the microscope, a great variety of curious forms. In most instances these "hairs" are of a dull black; but sometimes they have a silvery appearance. The forms assumed are occasionally of such a character that it is difficult to divest one's mind of the idea that they are organic structures.

The "hairs" are not uniform in thickness, but vary from the $\frac{1}{1000}$ of an inch in diameter, or even less, to perhaps the $\frac{1}{100}$ of an inch, or even more. Some are straight or only slightly curved (figs. 3, 4, 5); others are bent, distorted, and knotted in a variety of ways. The finer "hairs" are mostly smooth and cylindrical; but some have a granular appearance; and occasionally the granules are so large in proportion to the size of the "hair" as to present an irregular knotted appearance, like those forming the tuft of specimens such as fig. 10. On many of the larger "hairs" more or less distinct transverse markings may be seen (fig. 6); and these often become definite constrictions arranged in a very regular manner (fig. 7)—so much so that they closely resemble, on a small scale, the moniliform stems of the stone lilies or Encrinites, which are so well known in the Wenlock and Carboniferous Limestones. If one imagines a form such as this drawn out so as to increase the distance between each ridge, the constrictions being at the same time deepened, an appearance such as that given in fig. 8 would be produced. Forms like these are not at all rare. Each segment appears to consist of a bundle of tubes or rods close together where they spring from the segment below, but gradually spreading out as they pass upwards, and terminating abruptly at the rounded surface which forms the top of the segment (fig. 8^a, *). No definite openings could be seen upon the upper rounded surface; but it appeared slightly granular. This form reminds one strongly of certain kinds of tubular Polyzoa, only that it is so much smaller. Fig. 9 represents a form of which there are many varieties, all exhibiting a

series of regular ridges passing obliquely outwards from a middle line, which is either a groove, a ridge, or an irregular ridge broken up into segments, as in the figure. This specimen is not cylindrical, but flattened, and has the appearance of being two "hairs" placed side by side and joined to each other at intervals.

Specimens such as that given in fig. 11 do not seem to be so common as some of the others. At first sight this appears like a number of shots irregularly massed together; but upon closer inspection it will be seen that it consists, not of spheres, but of very sharply defined hemispheres, the flat surfaces of which are uniformly turned inwards.

Fig. 10 represents one of a number of forms which resemble minute drumsticks—the knobs of each consisting of a tangled mass of hairs and granules, and giving one the impression that it has been caused by an explosion.

With regard to the internal structure of this hair-like coke, some of the fibres when broken open appear vesicular, while others are solid and have much the aspect of a piece of charcoal. The solid "hairs" must, I think, have been formed in the first place as threads of semifluid carbonaceous material, and subsequently decomposed by being further heated. There is little room for doubting that the vesicular kind of "hairs" have been produced by a process of bubbling, caused by gases forcing their way through the semifluid carbonaceous matters in the manner intimated by Dr. Percy (*loc. cit.* p. 421). It is not difficult to imagine that such simple forms as those represented in figs. 3, 4, and 5 may have been formed in either of these two ways, or to understand how a series of bubbles formed rapidly one beyond another might produce the moniliform character of fig. 7, although one would scarcely have expected it to be so regular; but it is not easy to imagine how either process could have produced the regular oblique markings of fig. 9, or the bundles of tubes (?) seen in the specimen fig. 8. With regard to the hemispheres (fig. 11), one would like to know how it is that they are so sharply defined as *half*-spheres, instead of being spheres (or nearly so), as we might have expected them to be—that is, supposing that they are bubbles.

The figures which are given do not represent all the forms which may be seen; they were merely taken as good illustrative examples, the variations and intermediate conditions being well nigh endless.

A specimen of hair-like coke may be seen in the Museum of Practical Geology; but it does not exhibit the various forms alluded to above, in so marked a manner as that in the possession of Dr. Percy.

III. Mallet's *Volcanic Theory* "tested" by the Rev. O. Fisher. By ROBERT MALLET, F.R.S.*

IN July and August last the Rev. O. Fisher, in asking me for some explanations in reference to my paper "Rock-crushing and its Consequences" (Phil. Mag. for July 1875), with which I supplied him, informed me that he was preparing a further paper, in which he proposed to test my theory of the origin of volcanic heat and energy by some application of mathematical reasoning. I venture to subjoin an extract from my letter in reply addressed to him on the 30th of August last:—"I have no wish to dissuade you from any criticism of my volcanic views which may occur to you as important to make; but at the same time allow me to remark that those views of mine are greatly more dependent upon a large number of physical considerations than upon any mathematical ones, and I do not think that symbols or arithmetic are likely to throw any additional light upon the subject, however they may tend to confuse it. I replied to Mr. Hilgard's and your own objections because it seemed necessary that I should fill up a lacuna purposely left in my original paper, and not in the spirit of controversy; nor do I wish or intend to engage in any further controversy as to any objections that may be made to any of my views.

"Bacon has, I think, somewhere said that, with respect to large and complex questions, it is best to lay aside instant discussion and allow time and rumination to wisen us upon the subject. Time and the advance of science in the future will no doubt afford surer tests of the truth or falsehood of my views than we now possess; but in the existing state of terrestrial physics, partial objections, even if well founded, seem to me of little value or use."

Mathematical reasoning is an admirable and potent instrument for the discovery of truth when the data upon which it is founded are exact, sufficient, and such as we are sure exist in nature; but all its validity depends upon these data.

We know almost nothing as to the nature of the interior of our globe; and it is only in a very imperfect way that we can even imagine the conditions, highly complex as these undoubtedly must be, under which mechanical strains act upon it even within a few miles of the surface, where we may reasonably infer its materials and their arrangement to be highly complicated, and differing from point to point in their chemical and

* Communicated by the Author.

physical natures ; of the material and its arrangement at much greater depths we know absolutely nothing.

The theories propounded of the descent of glaciers present examples, now familiar to many, of mathematics misused, because as yet our knowledge of the physical nature of ice is so imperfect. This is still more true where mathematical calculation is attempted to be applied under conditions such as affect the interior of our globe (as compared with which the motions of glaciers are simplicity itself), as is done by the Rev. O. Fisher in his paper entitled "Mr. Mallet's Theory of Volcanic Energy tested," which appears in the *Philosophical Magazine* for October last.

I shall adhere to my resolution expressed in the above extract not to engage in any mathematical controversy in support of my views as to volcanic activity, but to leave it to the issue of advancing knowledge, when in time to come more extended and new forms of observation or experiment shall have afforded more certain physical data than we at present possess whereon to base our conclusions. In abstaining thus it must not be supposed that I admit the validity of the Rev. O. Fisher's conclusions, or that the pretentious title of his paper is in any wise justified by them. Passing by the earlier parts of his paper, as to which all that need be said may already be found in my paper in the *Philosophical Magazine* for July, the connected argument by which he professes to test my views commences at page 309. The physical data upon which it is founded have no real or probable existence in nature, and are in some instances in conflict with each other, while some of the numerous hypotheses involved in his calculation are not warranted by any thing set forth by me in my original paper (*Phil. Trans.* for 1873). For his own purpose he takes my small paper subsequently read (addition &c., read May 1874), in which upon certain hypotheses and suppositions I assigned limits of thickness to the earth's solid crust, as though it were part of my original paper and resting upon an equally assured base.

The Rev. O. Fisher assumes a rigid crust and rigid nucleus, that these are in contact at a definable spherical surface, that at this surface they adhere or stick together, and that when this adhesion is broken by tangential forces originating in contraction of the nucleus, the surfaces of contact drag over each other with an enormous resistance, which he supposes at the moment of rupture equal to the whole weight of the crust. This fanciful coefficient of adhesion and friction (μ and μ' in the author's formulæ) he professes to take from me ; but what I have assumed for sake of illustration only at pages 8-9

(Phil. Mag. July last), in reference to disintegrated material, affords no warrant for the Rev. O. Fisher's application of it under conditions essentially different, while for his coefficient of adherence, or for the existence of adherence at all, he offers no warrant whatever. The coefficient of friction, which I have assumed in illustration only, is not 0.75, but 0.5; and whether either of these be true or not, for the moderate pressures of a few pounds per square foot as in Morin's experiment, they cannot be true, and in fact involve a physical impossibility, where the pressure per unit of surface enormously exceeds the crushing-resistance of the material as in his case, where the pressure is that of a column 400 miles in height. Yet it is upon these data that his argument rests, and by which he manages to get rid of the largest portion of the work due to the descent of the crust, and so to prove the residue insufficient for the production of volcanic heat. There is nothing to warrant the supposition that a crust 400 miles thick, which is the value our author assumes for k , would be compressed equally throughout its depth or crush simultaneously throughout its thickness; nor can it be assumed that volcanic activity is found uniformly diffused throughout the depth of such a crust, but must be supposed, as I have shown in my original paper (§ 87), to be confined principally to the upper strata of the crust, where, as may easily be seen, in an elastic and flexible crust local lateral displacements may take place sufficient to produce crushing and volcanic action without any dragging of the crust as a whole over the nucleus.

If these data and others which I have not specified, as well as several assumptions which the paper involves, be false, as they undoubtedly are, then must the conclusions be false likewise, and this testing of my theory be but weighing it in a false balance.

But somewhat further on we find the author overthrowing, in the following sentence, the entire mathematical house of cards which he has with so much parade erected:—"If, however, as is more likely, the crust rests upon a fluid or viscous layer, the resistance to lateral motion will be much smaller; but we are not able to guess what it will be, so that we cannot *à priori* assign a value to μ " (page 316). Now, as the only conceivable assumption that we can make is that adopted by all physical geologists, namely that a solid crust passes by an intervening viscous layer into a hotter nucleus below it, so this statement on the part of the Rev. O. Fisher is to admit that his whole mathematical argument is baseless and worthless. It seems to me a notable example of the misuse of mathematics which Professor Huxley, in one of his addresses as President of the Geological Society, not less wittily than truly

illustrated, by saying that if we put peascods into the mathematical mill we cannot expect it to yield wholesome wheat-flour. Such ill-founded calculations do not advance, but retard truth; they do so especially when applied to such questions as are related to physical geology, upon which opinions are adopted by large numbers who know nothing of mathematics, and by whom mathematical symbols are too commonly taken as tests of truth, and upon whom their parade exercises a sort of fascination like that said to affect birds under the glance of the rattlesnake. Were this intended as a refutation of the Rev. O. Fisher's paper, and not merely to point out the invalidity of his conclusions resting on such infirm data, I might point to the physical impossibility which appears to me to be involved in the first part of the answer he has given to his own question, page 317, "If the work of descent of the crust is not transformed into the heat of volcanic energy, it may be asked what becomes of it?" He says part of it "is transformed into heat within the nucleus," his own assumption being that the nucleus itself is *hotter* than the heat of vulcanicity. But this, as well as the string of improbable suppositions not containing any thing new, with which the author endeavours to prop up old volcanic theories at the conclusion of his paper, I pass without remark.

IV. *The Second Proposition of the Mechanical Theory of Heat deduced from the First.* By C. SZILY, Budapest*.

THERE is not, and never will be, any theory which could dispense with fundamental hypotheses incapable of demonstration and explanation; still a theory must be regarded as more perfect the less it stands in need of such undemonstrable assumptions. The mechanical theory of heat, according to the present view of it, rests upon two propositions of this sort. The first (named after Mayer and Joule) is no other than the universal principle of the Conservation of Energy, in its application to heat.

The second proposition (that of Carnot and Clausius) cannot be expressed so simply, or be so readily fitted into the framework of a general physical principle, as the first-mentioned. This second proposition is formulated by Clausius as follows†:—

"Whenever a quantity of heat is converted into work, and the body through which the conversion is effected is finally

* Communicated by the Author. Translated from the *Mathematikai Ertekezések*, vol. iv. 1875. The original memoir was presented to the Hungarian Academy of Sciences, May 10, 1875.

† Pogg. *Ann.* 1854, vol. xciii.

found in its initial condition, another quantity of heat must simultaneously pass from a hotter into a colder body ; and the amount of the latter quantity of heat in proportion to the former depends only on the temperatures of the two bodies between which it passes, and not upon the nature of the body through which the transformation was effected."

Clearly a proposition so complicated, however accordant with the facts of experience, cannot be accepted as a simple axiom. Hence physicists were obliged to seek an hypothesis that could be more easily expressed, more distinctly represented, than the Second Proposition, and from which the latter could then be strictly deduced mathematically. In fact a series of such axioms for the deduction of the Second Proposition have been found.

Let us consider these in the order in which they have successively been proposed. For the facility of our survey we will arrange them in two groups. The first comprises those assumptions which refer to the behaviour of heat as an agent, without entering in detail into the nature of the motion which we name heat ; while the second group comprises those hypotheses which relate to the nature of the motion.

To the group of *thermic hypotheses* belong :—

(1) Clausius's axiom, proposed by him in his fundamental memoir (read in February 1850 before the Berlin Academy), and afterwards more completely presented* :—"*Heat cannot of itself pass out of a colder into a hotter body.*"

(2) Thomson's axiom †, which reads thus :—"*It is impossible, through the action of dead matter, to obtain mechanical work by cooling any natural substance below the temperature of the coldest of the surrounding objects.*"

(3) Clausius's second hypothesis, expressed,‡ in his memoir relative to disgregation †, as follows :—"*The mechanical work which heat can perform in any change of arrangement of a body is proportional to the absolute temperature at which the change takes place.*"

(4) Belpaire's axiom §, viz. :—"*If the temperature be infinitesimal, the quantity of energy converted into work by an isothermal transformation must likewise be infinitesimal.*"

These are the *thermic* hypotheses which, to my knowledge, have been proposed for the derivation of Carnot and Clausius's principle. The second group comprises the *dynamic* hypotheses proposed for the same purpose, viz. :—

(5) The hypothesis of "molecular vortices," set forth in

* Pogg. Ann. 1850, vol. lxxix. ; Phil. Mag. [IV.] vol. ii.

† Edinb. Trans. vol. xx., and Phil. Mag. [IV.] vol. iv.

‡ Pogg. Ann. 1862, vol. cxvi. § Bull. de Belg. 1872.

1850 by Rankine *, according to which the particles of bodies move in circular currents.

(6) The hypothesis of circulating currents ("circulating streams of any figure whatever"), developed likewise by Rankine, in 1851, and considerably simplified by him in 1855 †.

(7) The hypothesis of periodic motions, made use of by Boltzmann, in 1866, in order to deduce the Second Proposition ‡.

(8) The hypothesis of quasi-periodic motions, which Clausius first advanced in 1871 §, and has since more fully developed in several large memoirs.

Of all the above-mentioned hypotheses, the axioms of Clausius and Thomson found the most acceptance, as being the most plausible.

But now arises the question whether a new hypothesis is needed for the deduction of the Second Proposition, and whether it cannot be deduced with mathematical rigour by means of the First Proposition alone. If this can be done, the theory of heat will be really based on a single proposition, on the universal principle of the conservation of energy.

At present this problem is not merely unsolved, but has scarcely been taken into consideration. As far as I know, only Rankine and Clausius have expressed any opinion on this question, and the latter merely by the way. Rankine pronounces as follows:—"Carnot's law is not an independent principle in the theory of heat, but is deducible as a consequence from the equations of the mutual conversion of heat and expansive power" (Phil. Mag. [IV.] vol. vii.), and adds that he has shown this in the first part of his treatise. He forgets, however, that in the part cited he occupies a very hypothetical stand-point, in that, *inter alia*, he defines temperature thus:—"The temperature is a function of the square of the velocity of the molecular vortices, divided by the coefficient of elasticity of the atmospheres of the atoms." It was very natural that a deduction based on such a complicated hypothesis was accepted by no one. Rankine's consequence is, accordingly, a mere assertion, of which he has not furnished the demonstration.

Clausius alludes to the above question quite incidentally, in a lecture delivered by him at the 41st Meeting of the German Natural Philosophers and Physicians, at Frankfort. He expresses himself to this effect:—"Yet there is a second principle, which is not yet contained in that (the First Proposi-

* Edinb. Trans. vol. xx.

† Phil. Mag. [IV.] vol. xxx.

‡ *Sitzungsberichte der Wiener Akademie*, vol. liii.

§ Pogg. *Ann.* vol. cxlii.

tion), but requires a special demonstration, and which is equally important with the other, because it is only the combination of the two that can form a complete basis for the mechanical theory of heat."

It is evident that Clausius's statement directly contradicts the assertion of Rankine. The latter maintains that the Second Proposition can be deduced from the First; while Clausius says that the Second is not contained in the First.

The purpose of the present paper is to examine this important question, and to prove that, without any further hypothesis, the Second Proposition can be deduced direct from the First.

I.

Every material body can be regarded as an aggregate of an immense number of material points, which, under the influence of internal and external forces, move according to certain unknown laws. We will not bind ourselves to any hypothesis concerning the nature of this internal motion; our sole and single assumption shall be that the particles are not at rest, but in some kind of motion.

To characterize this motion, or, in other words, to determine the thermic condition of a body, it is known that two independent variables are necessary. Let them be denoted by ξ and η ; then the state of the body remains unaltered so long as neither ξ nor η changes; but as soon as either ξ or η or both together undergo a change, the state of the body in question changes at the same time.

As long as the thermic condition of a body remains unchanged, so long do, according to my conception, *the total living force (T) by itself, and the total potential energy (U) by itself, remain constant.*

If, therefore, the state of the body is given by the two independent variables ξ and η , then, as long as $\delta\xi=0$ and $\delta\eta=0$, in like manner

$$\delta T=0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

and

$$\delta U=0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Let the mass of any material point whatever of the body be m ; further, at the time $t=0$ let x, y, z be its rectangular co-ordinates, x', y', z' the components of the velocity, and x'', y'', z'' the acceleration-components. Already in the first instant—say, after the time δt , the position, velocity, and acceleration of the point will be different, viz. $x+\delta x \dots, x'+\delta x' \dots, x''+\delta x''$. But, from equation (1), must

$$\delta T=\Sigma m(x'\delta x'+y'\delta y'+z'\delta z')=0; \quad . \quad . \quad . \quad . \quad (1A)$$

and, according to equation (2),

$$\delta U = -\Sigma m(x''\delta x + y''\delta y + z''\delta z) = 0. \quad . \quad . \quad (2A)$$

By forming the two latter equations we readily obtain a relation between the time-variations and the variations of the coordinates; for if (1A) and (2A) be multiplied by dt and added, we shall have

$$\Sigma m(x'\delta x' + \dots - x''\delta x - \dots)dt = 0,$$

or, in accordance with the signification of x' and x'' ,

$$\Sigma m(dx\delta x' + \dots - dx'\delta x - \dots) = 0.$$

Now

$$dx'\delta = dx(x'dx) - x'd\delta x;$$

consequently

$$\Sigma m(dx\delta x' + \dots + x'\delta dx + \dots) = d\Sigma m(x'\delta x + \dots).$$

Further,

$$dx\delta x' + x'\delta dx = \delta(x'dx) = \delta(x'^2 dt);$$

consequently

$$d\Sigma m(x'\delta x + \dots) = \delta(2Tdt).$$

As long as the state of the body remains constant, T also is constant; consequently, after indefinite integration,

$$\Sigma m(x'\delta x + \dots) = 2T\delta t + \text{constant}.$$

But now, for $\delta t = 0$,

$$\delta x = 0 \dots \dots;$$

consequently

$$\Sigma m(x'\delta x + y'\delta y + z'\delta z) = 2T\delta t. \quad . \quad . \quad . \quad . \quad (3)$$

Equations (1), (2), and (3) remain valid so long, and only so long, as the state of the body is unaltered.

II.

We will now pass to the consideration of the changes of state.

Let the initial state of the body (the state before the change) be defined by the constants ξ_0 and η_0 . Let the coordinates of any particle m in the instant $t=0$ be x_0, y_0, z_0 ; the velocity-components, x'_0, y'_0, z'_0 ; the total *vis viva*, T_0 ; and the total potential energy, U_0 . If the state of the body were to remain unchanged, then equations (1), (2), and (3) would continue to hold good.

But now, from the time $t=0$ onward, we will convey to the body successive quantities of energy. In consequence of this,

the *vis viva* and potential energy of the body will be augmented, and, in general, external work will be done. In the measure in which we add more and more energy to the body, the state becomes also more and more changed. Assume that the series of successive changes is regulated in such manner that the condition-variables satisfy at every instant the equation $\eta=f(\xi)$, or, in other terms, that the succession of the changes of state (say, the path of the state) is given by the function f . We will continue the importation of energy until the condition-variables, corresponding to the relation $\eta=f(\xi)$, take the values ξ_1 and η_1 . Let the quantity of imported energy be then Q . With the cessation of the importation of energy change of state ceases also; henceforward the variables ξ_1 and η_1 , and with them the *vis viva* T_1 and potential energy U_1 , retain their present values.

We have, consequently, conducted the body in a definite manner from the initial state $(\xi_0\eta_0)$ into the final one $(\xi_1\eta_1)$, and meanwhile conveyed to it the energy Q . Given the initial state $(\xi_0\eta_0)$, final state $(\xi_1\eta_1)$, and the path of the changes of state, the total energy Q to be conveyed to the body is also unambiguously determined. Whether the importation of energy takes place more quickly or more slowly, the sum of the imported energy remains unchanged, if only the initial point, the end-point, and the nature of the path remain unaltered; in other words, *the initial and final states and the path of the changes completely determine the total energy Q to be imported; while the duration of the transition is not determined thereby.* With the same Q the duration of the transition, corresponding to the mean velocity of the importation of energy, may take any value whatever between 0 and ∞ . Let the duration be denoted by t . The length of this duration, or the value of t , depends entirely upon how we fix the mean velocity of the importation of energy; consequently the choice of what the magnitude of t shall be, is entirely at our disposal.

Let us now conduct the body from the same initial state $(\xi_0\eta_0)$ into the same final state $(\xi_1\eta_1)$, but so that the present series of changes (the second path of the state) shall be only infinitesimally different from the just-described series (the first path)—that is, so that the form of the function f in the equation $\eta=f(\xi)$ undergoes an indefinitely little alteration. The importation of energy now commences not at the instant $t=0$, but the infinitesimal time δt later. Hence the coordinates of the particles m at the commencement of the present change of state will be no longer x_0, y_0, z_0 , but different by $\delta x_0, \delta y_0, \delta z_0$; and the same will be the case with the components of the velocity and acceleration. But during this time δt the state of the

body has remained unchanged; so that equation (3) still retains its validity; consequently

$$\Sigma m'(x'_0 \delta x_0 + y'_0 \delta y_0 + z'_0 \delta z_0) = 2T_0 \delta t. \quad (4)$$

At the end of the time δt the conveyance of energy commences, corresponding to the former in such sort that *the body, after an equal period t, arrives at the same final state as by the first path.* Let the total quantity of energy now communicated be accordingly $Q + \delta Q$.

On the second path, where the change of state began δt later, it will also end δt later—that is, at the instant $t + \delta t$. At this instant the coordinates, velocities, and acceleration-components are not the same ($x_1 \dots, x'_1 \dots, x''_1 \dots$) as they were at the end of the first transition, at the instant t , but differing from them by $\delta x_1 \dots, \delta x'_1 \dots$, and $\delta x''$. But now, on the one hand, $x_1 \dots$, and, on the other, $x_1 + \delta x_1 \dots$ represent the coordinates of the same particle in the same state, and only so far differ from one another that the first correspond to the instant t , and the latter to the instant $t + \delta t$. Therefore equation (3) is applicable to these variations also; that is,

$$\Sigma m(x'_1 \delta x_1 + y'_1 \delta y_1 + z'_1 \delta z_1) = 2T_1 \delta t. \quad (5)$$

III.

Equations (4) and (5) refer only to the commencement and termination of the change of state, and cannot be applied to the entire course of the transition. We must seek another relation, one which remains valid at any and every stage of the alteration.

We will assume that on the first path the body, during the time τ , has arrived at a certain state ($\xi\eta$), which we will briefly designate by the name “state A_1 .” In this state let the total *vis viva* be T , the potential energy U , and the coordinates, velocities, and acceleration-components of the particle m be $x \dots, x' \dots$, and $x'' \dots$.

On the second path the body comes in the course of the time τ (therefore in the instant $\tau + \delta t$) into a certain state ($\xi + \delta\xi, \eta + \delta\eta$), which we will name “state A_2 .” The quantities belonging to A_2 differ only by their variations from those which belong to A_1 . The particle m is now not in the position $x \dots$, but removed δx from it; and in like manner the velocity- and acceleration-components have suffered an infinitesimal variation $\delta x' \dots, \delta x''$.

Let us now determine how much energy must be communicated to the body to conduct it, following the path denoted by the displacements $\delta x, \delta y, \delta z$, out of state A_1 into state A_2 .

If $\delta\epsilon$ is the amount of energy sought, then

$$\delta\epsilon = dT - \Sigma m(x''\delta x + y''\delta y + z''\delta z),$$

or also

$$\delta\epsilon = \Sigma m(x'\delta x' + \dots - x''\delta x - \dots);$$

multiplying by dt , and taking into consideration the signification of $x'\dots, x''\dots$, we find

$$\delta\epsilon \cdot dt = \Sigma m(dx\delta x' + \dots - dx'\delta x - \dots).$$

By simple transformations we get

$$\delta\epsilon \cdot dt = \delta \Sigma m(x'dx + \dots) - d \Sigma m(x'\delta x + \dots).$$

Integrating this equation between the limits corresponding to the initial and final states, we have

$$\int_0^t \delta\epsilon \cdot dt = \delta \int_0^t \Sigma m(x'dx + \dots) - \Sigma(x'_1\delta x_1 + \dots) + \Sigma m(x'_0\delta x + \dots).$$

Transforming the integral on the right-hand side, taking into account equations (4) and (5), gives

$$\int_0^t \delta\epsilon \cdot dt = 2\delta \int_0^t T dt - 2T_1\delta t + 2T_0\delta t.$$

As T_1 and T_0 are constant, the equation can also be written thus—

$$\int_0^t \delta\epsilon \cdot dt = 2\delta \int_0^t (T - T_1 + T_0) dt. \quad \dots \quad (6)$$

In a definite actually effected passage out of $(\xi_0\eta_0)$ into $(\xi_1\eta_1)$ the quantity of energy is a certain function of the time. *The passage, however, from the same initial to the same final state, by the same path, can, with respect to time, be executed quite arbitrarily*; consequently $\delta\epsilon$ is a different function of the time, according to the velocity of the importation of energy. By suitably selecting this velocity, any indefinitely small value whatever may be assigned to

$$\int_0^t \delta\epsilon \cdot dt.$$

Let the velocity of the importation of energy be such that the period of the transition shall satisfy the following equation of condition—

$$\int_0^t \delta\epsilon \cdot dt = t \cdot \delta Q,$$

where δQ signifies the amount to which more energy would be required by the transition on the second path than by the transition on the first path.

To distinguish this period thus defined from the former arbitrary one, we will designate it by i ; then

$$\int_0^i \delta \epsilon . dt = i \delta Q, \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

and

$$i \delta Q = 2 \delta \int_0^i (\bar{T} - T_1 + T_0) dt;$$

or, \bar{T} denoting the mean value of T during the time i , and putting for shortness

$$\bar{T} - T_1 + T_0 = \mathfrak{T},$$

we have

$$i \delta Q = 2 \delta (i \mathfrak{T}),$$

and hence

$$\frac{\delta Q}{\mathfrak{T}} = 2 \delta \log (i \mathfrak{T}). \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

This equation agrees perfectly, in form, with that to which, in a former memoir *, I reduced Hamilton's principle. I then started from the hypothesis that

$$\Sigma m(x'_1 \delta x_1 + \dots) = \Sigma m(x'_0 \delta x_0 + \dots).$$

If we drop this hypothesis, we see that the quantity \mathfrak{T} takes the place of the \bar{T} which occurs in the equation mentioned. In the present deduction the other limiting assumption is also set aside, viz. that the acting forces have a force-function.

Equation (8) specifies how much energy must be conveyed to the body, in order that, instead of the path $\eta = f(\xi)$, it may follow another deviating indefinitely little from this. The same equation (8) we also apply to the case in which we seek the amount of the energy which must be conveyed to the body in order that it may pass out of a given state $(\xi \eta)$ into another state infinitesimally different $(\xi + d\xi, \eta + d\eta)$. In correspondence with this, we substitute for the symbol of variation the differential-symbol, and write

$$\frac{dQ}{\mathfrak{T}} = 2 d \log (i \mathfrak{T}). \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

So long as the body continues in the same state, $dQ = 0$, and

$$\mathfrak{T} = T - T + T = T.$$

\mathfrak{T} being constant, i must also be constant. For each determinate $(\xi \eta)$ state of the body, \mathfrak{T} and i have a certain determinate value, which remains constant as long as the body is in the

* Pogg. *Ann.* vol. cxlv. p. 300; *Phil. Mag.* [IV.] vol. xliii. p. 339.

same state. In another state, i and \mathfrak{T} have other values, which again are constant as long as the new state is unchanged.

If the infinitesimal quantity of energy dQ be conveyed to the body, the variables $\mathfrak{T} = T$ and i change also infinitesimally; consequently

$$\mathfrak{T} = \bar{T} - (T + dT) + T;$$

and recollecting that in this case \bar{T} also can only be infinitesimally different from T , we obtain, for an infinitesimal change of state,

$$\mathfrak{T} = T,$$

wherein, for each element of energy dQ received, that *vis viva* $\mathfrak{T} = T$ must be employed which the body has directly on the reception of that element.

If now we integrate equation (9) between the limits corresponding to the initial and final states, we have

$$\int_0^1 \frac{dQ}{\mathfrak{T}} = 2 \log \frac{(i\mathfrak{T})_1}{(i\mathfrak{T})_0}.$$

Extending the integral to a complete circular process gives

$$\int \frac{dQ}{\mathfrak{T}} = 0, \quad (10)$$

This equation corresponds perfectly with the following,

$$\int \frac{dQ}{T} = 0. \quad (11)$$

which expresses the Second Proposition for reversible circular processes. In equation (10) \mathfrak{T} denotes the total *vis viva* which the body possesses directly on the reception of the element of energy dQ ; in equation (11), T denotes the absolute temperature which the body possesses directly it receives the heat-element dQ . If it be assumed (as it usually is) that the absolute temperature is a quantity proportional to the total *vis viva*, equation (10) immediately changes into equation (11).

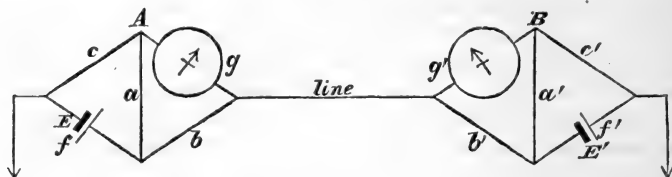
In the preceding considerations we have endeavoured to keep aloof from every hypothesis, basing our deductions solely upon the principle of the Conservation of Energy. We believe we have thereby proved that the Second Proposition can be deduced from the First without any further hypothesis.

Budapest, June 15, 1875.

V. *On Duplex Telegraphy.* By OLIVER HEAVISIDE.**The Bridge System.*

A THEORETICAL diagram of the arrangement of the conductors for duplex telegraphy by the bridge system is given in fig. 1. g and g' are the receiving apparatus, which

Fig. 1.



may be of any kind, f and f' the batteries, and a, b, c, a', b', c' are resistances. The letter attached to any branch will be used to represent the resistance of that branch. The two branches f and f' are preserved of constant resistance by mechanical means, whether the batteries are in or out of circuit. The object of the above arrangement of conductors is to enable two stations, A and B, to signal each other at the same time through a single line without mutual interference; and this is accomplished by adjusting the six resistances, a, b, c, a', b', c' , so that

$$a : b :: c : x$$

and

$$a' : b' :: c' : x',$$

where x is the resistance outside station A, *i. e.* the resistance of the line *plus* the resistance of station B's apparatus, and x' is the resistance outside station B. When the above proportions hold good, f and g are conjugate, likewise f' and g' ; hence each station working alone produces no current in its own receiving instrument; and when they are both signalling simultaneously, the currents in the receiving instruments are the algebraical sums of the currents which would be separately produced; thus station A gets only B's signals, and station B receives A's signals alone. It is obvious without any mathematical demonstration that the above is true whatever may be the direction and strength of the currents, provided only that the two conditions $ax = bc$, $a'x' = b'c'$ are satisfied, and that these are the sole necessary conditions for duplex telegraphy when the line has so little electrostatic capacity that the trans-

* Communicated by the Author.

ient currents due to that cause are inappreciable. When the capacity of the line cannot be neglected, it may be perfectly balanced by distributing artificial capacity along the resistance c with the same uniformity it has along the line; and this may be approximated to by subdividing the resistance and required capacity as much as possible. If l is the resistance and l_1 the capacity of the line, and c_1 the required capacity, then the condition of balance as regards capacity is $cc_1 = ll_1$.

Since each station has three adjustable resistances, a , b , and c , and they are connected by the single relation $ax = bc$, it follows that any two of them may be taken arbitrarily and balance made with the third. Thus we may take b and c as independent variables, and eliminate a . The question then arises, in what respects an arrangement in which the resistances b and c have particular values differs from another in which b and c have other values. There are three principal differences: first, the received currents will be in general different; next, the balances will be of different degrees of sensitiveness, so as to be more or less affected by changes in the resistance of the line; and, lastly, different amounts of artificial capacity will be required to produce a balance with respect to the capacity of the line. Since a duplex apparatus is generally set up for permanent use, it is clearly of the first importance to obtain the maximum current with a given receiving instrument and battery. On cables this is quite a minor consideration, on account of the great delicacy of the instruments employed. But duplex telegraphy has not hitherto been very successful on cables; whereas on land lines, where such delicate instruments would be quite out of place, and much larger batteries are employed, the cost of the current is considerable, and it is desirable to get as much out of it as possible. I shall therefore in the first place endeavour to discover what the actual magnitudes of the resistances a , b , and c should be to render the received current a maximum,—and when that is done, consider the sensitiveness of the resulting arrangement, or its liability to disturbance.

In order to avoid useless complication, I shall suppose that the line is perfectly insulated, and that the receiving instrument at each station has the same resistance, and likewise the battery at each station; i. e. $f = f'$ and $g = g'$. This is very nearly fulfilled in practice; for the same description of instrument and battery is generally used at each end of a line. Then symmetry tells us that the resistances a , b , c should be the same at each station; or rather there is no reason why they should be different; and besides, if we do not make use of this simplification, the problem will become almost intractable.

Let E be the electromotive force of each battery, and G the

current each station receives from the other through its receiving instrument g , and let both A and B send the same current to line. Then, from the identity of the arrangement at each end, there will be no current in the line, which may be removed without influencing the currents in the other conductors. Thus we find

$$G = \frac{E}{f+c + \frac{a(b+g)}{a+b+g}} \times \frac{a}{a+b+g}$$

or

$$G = \frac{Ea}{(f+c)(b+g) + a(f+c+b+g)}. \quad \dots (1)$$

Now this expression for the strength of the received current contains the constants E, f , and g , and the variables a, b , and c . The last two are independent; but the first is a function of all the resistances; for $a = \frac{bc}{x}$, and

$$x = l + \frac{gbx(c+f) + cfx(g+b) + bc(g+c)(b+f)}{(g+b)(c+f)x + bc(g+b+c+f)}. \quad \dots (2)$$

This gives a quadratic equation for the determination of x , which, however, it is unnecessary to effect. By eliminating $a = \frac{bc}{x}$ from (1), we have

$$G = \frac{Ebc}{(f+c)(b+g)x + bc(f+c+b+g)}. \quad \dots (3)$$

We have to make G a maximum with respect to b and c ; and therefore we must have $\frac{dG}{db} = 0$ and $\frac{dG}{dc} = 0$. Thus we have the following conditions:—

$$\left. \begin{aligned} gx(f+c) - b^2c &= b(f+c)(b+g) \frac{dx}{db}, \\ fx(b+g) - bc^2 &= c(f+c)(b+g) \frac{dx}{dc} \end{aligned} \right\} \quad \dots (4)$$

The only difficulty now lies with the complex function x . It would be most natural to obtain $x, \frac{dx}{db}$, and $\frac{dx}{dc}$ as functions of b, c, f, g , and l , and then find the values of b and c , in terms of the constants f, g , and l , which make G a maximum. But it will be found impossible to obtain an explicit solution in this manner, owing to the high degree of the final equations. How-

ever, a simple solution may be obtained in terms of f , g , and x , the external resistance. Thus, differentiating (2) with respect to b and then solving for $\frac{dx}{db}$, we obtain

$$\frac{dx}{db} = \frac{\{gx(c+f) + bc(c+g)\}^2 + cx(bc-fg)^2}{\{x(g+b)(c+f) + bc(g+b+c+f)\}^2 + bc(bc-fg)^2}; \quad (5)$$

and since the right-hand side of (2) is unaltered by interchanging f and g , and b and c , we can find $\frac{dx}{dc}$ by making these changes in (5). Thus

$$\frac{dx}{dc} = \frac{\{fx(g+b) + bc(b+f)\}^2 + bx(bc-fg)^2}{\{x(g+b)(c+f) + bc(g+b+c+f)\}^2 + bc(bc-fg)^2}. \quad (6)$$

Equations (4), (5), and (6) must now be manipulated to obtain b and c in terms of f , g , and x . After going through the usual algebraical drudgery, which it is unnecessary to give here, we obtain

$$\left. \begin{aligned} b &= \sqrt{\frac{xg}{x+g}}(x+f), & c &= \sqrt{\frac{xf}{x+f}}(x+g), \\ \text{whence} & & & \\ a &= \sqrt{fg}. \end{aligned} \right\} \quad (7)$$

It will be found that these values of a , b , and c make the received current a maximum with any given battery, receiving instrument, and line. The strength of the received current is then

$$G = \frac{E}{x+f+g + \sqrt{fg} + (\sqrt{xf} + \sqrt{xg})\left(\sqrt{\frac{x+f}{x+g}} + \sqrt{\frac{x+g}{x+f}}\right)}, \quad (8)$$

or, which is the same,

$$G = \frac{E}{a+b+c+f+g+x+b\sqrt{\frac{f}{g}}+c\sqrt{\frac{g}{f}}}.$$

It will be observed that in the above solution (7) one of the resistances (a) is independent of x , and is the same for all lines with the same receiving instrument and battery. Since balance is always obtained in practice by adjusting one of the resistances, say c , it follows that only the other two, a and b , need be calculated. This is readily done for a ; but for b it is more difficult, since x cannot be simply expressed in terms of

f , g , and l , the equation to determine it being of the sixth degree, viz.

$$l = \frac{(x^2 - fg) \{ 2\sqrt{xf} + 2\sqrt{xg} + \sqrt{(x+f)(x+g)} \}}{(f+g+x + \sqrt{fg}) \sqrt{(x+f)(x+g)} + (2x+f+g)(\sqrt{xf} + \sqrt{xg})}.$$

This theoretical difficulty, however, is of no practical importance, since the value of x can be determined as closely as necessary in the very act of adjusting the instruments for duplex working for the first time. For long lines, x may be considered equal to $l + \sqrt{fg}$. It is actually rather greater.

The next thing to be considered is to what extent this arrangement is liable to disturbance from variations in the resistance of the line. Now, as I have shown in a former paper (Phil. Mag. Feb. 1873), the values of a , b , and c , which make the most sensitive balance for measuring a resistance x with a battery of resistance f and galvanometer of resistance g , are precisely the same as those given in equations (7) above as giving the maximum current in duplex working. We are thus at once led to the conclusion that the arrangement of Wheatstone's bridge for duplex telegraphy which gives the maximum received current at both stations, is also the arrangement which is most easily disturbed by variations in the resistance of the line. We may show this otherwise. When $a : b :: c : x$, station A, when sending alone, produces no current in his instrument. Let now the external resistance x be changed to x' , then A sending alone will produce a current C_1 in his instrument, of strength

$$C_1 = \frac{Ebc(x' - x)}{b(c+x) + x(c+x')} \cdot \frac{1}{\left\{ \frac{b(c+x)(c+x')}{b(c+x) + x(c+x')} + g \right\} \left\{ \frac{c(b+x)(b+x')}{b(c+x) + x(c+x')} + f \right\}} - \left\{ \frac{bc(x' - x)}{b(c+x) + x(c+x')} \right\}^2$$

If another arrangement be made in which b and c are altered to b' and c' , and the current be now C_2 , then C_2 may be found by changing b into b' and c into c' in the expression for C_1 ; and the ratio $\frac{C_1}{C_2}$ when $x' - x$ is small will express the relative sensitiveness of the two arrangements. In the limit, when $x' - x = 0$, we have

$$\frac{C_1}{C_2} = \frac{\frac{bc}{b(c+x) + x(c+x)} \left\{ \frac{c(b+x) + f(c+x)}{b(c+x) + x(c+x)} \right\}}{\frac{b'c'}{b'(c'+x) + x'(c'+x)} \left\{ \frac{c'(b'+x) + f(c'+x)}{b'(c'+x) + x'(c'+x)} \right\}};$$

from which we see that the sensitiveness of any exact balance to disturbances in the resistance of the line, either in duplex working or in testing, is proportional to

$$\frac{bc}{\{b(c+x)+g(b+x)\} \{c(b+x)+f(c+x)\}};$$

and this expression is a maximum when b and c have the values given in (7) above.

Hence it is perfectly hopeless to find any arrangement of Wheatstone's bridge for duplex telegraphy which shall give the maximum received current at both stations and at the same time be least liable to disturbance. Generally speaking, the more sensitive the balance the stronger the received current.

Since x , the resistance external to one station, includes the resistance at the other station, any alteration of adjustment at one station will theoretically cause a disturbance in the other station's balance; and it is true that an infinite series of successive adjustments must be made by each station to reobtain an exact balance whenever balance is disturbed. But these alterations are so excessively small that practically they have no existence. By making $bc=fg$ and adjusting solely by the resistance a , each station's balance becomes independent of the other's; but this is introducing a greater difficulty to avoid a lesser and inappreciable one, since to keep $bc=fg$ frequent measurements would have to be made of f , the battery-resistance, a variable quantity; and besides, such an arrangement would not give the maximum current, as is evident from equations (7).

The above investigations apply to any instrument, battery, and line, and therefore admit of immediate practical application in any particular case. There are, however, two principles frequently made use of by theoretical writers on electric circuits:—first, that if the space to be filled with wire in a galvanometer or relay is fixed, the greatest strength of signal is obtained when the wire is of such a size that its resistance equals the external resistance; and next, that if the quantity of metallic surface of a battery is fixed, and also the distance between the plates in each cell, to obtain the maximum current the cells should be of such a size that the total resistance of the battery equals the external resistance. These principles do not often admit of practical application in telegraphy; but we may just see to what they lead us when we apply them to duplex working with the bridge. We shall have the following equations to determine f and g :—

$$x^2 - 3g(x+f) + 2\sqrt{fg}(x+g) = 0,$$

$$x^2 - 3f(x+g) + 2\sqrt{fg}(x+f) = 0.$$

Either of these equations by itself can be made use of to determine g when f is constant, or f when g is constant. When they are combined, we have

$$a=b=c=f=g=x.$$

Now, although this result can be applied to the construction of instruments for testing purposes where x is constant, there is one insuperable difficulty that prevents its use in duplex working; and that is, x becomes infinite. We can only conclude that the finer the wire of the relay and the greater the number of convolutions, the smaller the cells are made, and the greater their number, the greater will be the strength of the signals—a fact which might be safely predicted without mathematical examination.

A comparison of the strength of the received current in ordinary single working and duplex working with the bridge will be interesting. In single working the received current is

$$\frac{E}{f+g+l}; \quad \dots \dots \dots (9)$$

and in duplex working, when the arrangement is such as to give the maximum current, its value is given in equation (8) above as

$$G = \frac{E}{x+f+g + \sqrt{fg} + (\sqrt{xf} + \sqrt{xg}) \left(\sqrt{\frac{x+f}{x+g}} + \sqrt{\frac{x+g}{x+f}} \right)},$$

where x is the external resistance, rather greater than $l + \sqrt{fg}$. When l is very great compared with f and g , these expressions (8) and (9) are nearly equal, the duplex current being a little less than the other. (In the extreme case $f=0$, $g=0$, they are identical.) Numerical comparison, taking the most general values of f and g occurring in practice, will show that the duplex current is about one half or one third the strength of the current obtained when the same instruments and batteries are used for single working; so that in general more than double the electromotive force will be required to obtain signals of the same strength in both cases.

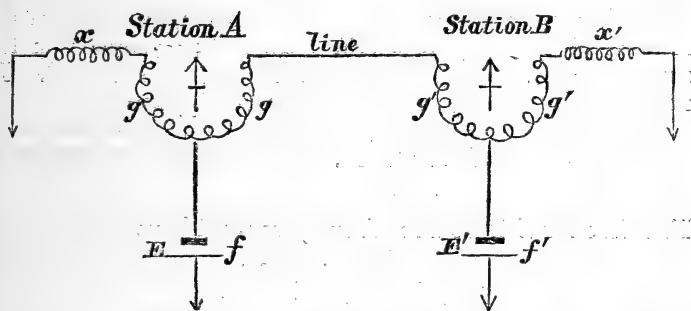
The third principal difference between one arrangement of the bridge and another, viz. that different amounts of artificial capacity are required, is of some importance as regards cables. Condensers of large capacity are such cumbrous and expensive affairs; that the smaller the artificial capacity can be conveniently made the better. Now c_1 , the required capacity, equals $\frac{l}{c} l_1$, where l_1 is the capacity of the line; consequently, to make

c_1 as small as possible, c must be as large as possible; and this will occasion a great loss of working current. This, however, will be of little consequence with the delicate instruments used on cables, if ever duplex telegraphy is successful on them.

The Differential System.

Any instrument may be used in the bridge system without alterations being made; but in the differential system the coils must be differentially wound, or some equivalent device employed, so that two currents, one passing through each coil, may annul each other's action on the magnet or cores within them. On the other hand, only one balancing resistance is required, instead of the three in the bridge system. The following diagram (fig. 2) is a theoretical view of the differential

Fig. 2.



system. g, g at station A and g', g' at B are the coils of the receiving instruments, f and f' the batteries, and x, x' the balancing resistances. We shall suppose, as is usually the case, that $f=f'$ and $g=g'$. To find the strength of the signals each station receives from the other, let both send the same current to line; then, from the identity of the arrangement at each end, there will be no current in the line, in the right-hand coil at station A, and in the left-hand coil at B. Therefore, if S is the strength of the signal,

$$S = \frac{Em}{f+g+x}, \quad \dots \dots \dots (10)$$

where E is the electromotive force of the battery, m the strength of signal produced by the unit current circulating through a single coil of the receiving instrument, and x the external resistance. The value of x is

$$x = l + g + \frac{f(g+x)}{f+g+x}, \quad \dots \dots \dots (11)$$

or

$$x = \frac{1}{2} \{ l + \sqrt{(l+2g)(l+2g+4f)} \}.$$

In single working with the same instruments and batteries the strength of the signals is

$$\frac{2Em}{f+2g+l} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (12)$$

when the current passes through both coils in succession. When f , the battery resistance, is very small, we see from (10) and (12) that the strength of the signals in duplex working is nearly one half their strength in single working with the same instruments, since x is a little greater than $l+g$.

Since there is only one balancing resistance at each station in this system, there is only one arrangement possible with a given receiving instrument and battery, leaving out minor details. We may, however, inquire what the resistance of the instrument should be to obtain the strongest signals on the supposition that the space to be filled with wire is fixed. In such case m will vary as the square root of g , and

$$S \propto \frac{E \sqrt{g}}{f+g+x}.$$

Therefore for S to be a maximum, we must have

$$f+g+x = 2g \left(1 + \frac{dx}{dg} \right);$$

and we find from (11)

$$\frac{dx}{dg} = \frac{(f+g+x)^2 + f^2}{(f+g+x)^2 - f^2};$$

therefore

$$g = \frac{1}{3} \{ -(x+f) + \sqrt{(f+2x)^2 + 4fx} \} \quad . \quad (13)$$

is the best resistance for each coil of the receiving instrument.

When $f=0$, $g = \frac{x}{3}$. Now (13) is identical with Weber's formula for the resistance of each coil of a differential galvanometer to obtain the maximum sensitiveness at a balance; thus again we see, just as in the bridge system, the arrangement in which both stations get the strongest signals is also the most sensitive balance, and most liable to disturbance from variations in the external resistance.

Weber's formula (13) admits of considerable simplification if we arrange the battery so as to obtain the maximum current,

by making $f = \frac{g+x}{2}$. We then have

$$g = \frac{x}{2}, \quad f = \frac{3x}{4}$$

as the best resistances for each coil and the battery. Now although this admits of application in testing with the differential galvanometer (when x is constant), yet it cannot be applied to duplex working, since x becomes infinite, which may receive an interpretation similar to that in the corresponding case of Wheatstone's bridge.

It is an interesting practical question whether with a given instrument and battery it is possible to obtain stronger signals by the bridge than by the differential system. To make the comparison fairly, in the former case the arrangement must give the maximum current. In the differential system the strength of the signal is (10)

$$\frac{Em}{f+g+x},$$

and in the bridge system

$$\frac{2Em}{f+x+2g+\sqrt{2fg}+(\sqrt{xf}+\sqrt{2xg})\left(\sqrt{\frac{x+f}{x+2g}}+\sqrt{\frac{x+2g}{x+f}}\right)}, \quad (14)$$

which is obtained from (8) by changing g into $2g$ and multiplying by $2m$. In (10) and (14) x has not the same signification; but the difference is not great. Effective comparison can easily be made numerically in any particular case. As general results, we may say that when f and g are very small in comparison with l , the advantage is in favour of the bridge system; but when f and g are taken larger, the advantage becomes rapidly in favour of the differential system. It may also be observed that in the latter the strength of signal is always less than one half the strength when the same instruments are used for single working, whereas in the former system the strength of the signal may be, but generally is not, more than one half.

If the practical success of duplex telegraphy were dependent on the continuous maintenance of an exact balance at each station, then would duplex telegraphy exist only on paper. The variations, sometimes large and rapid, which are always taking place in the resistance and insulation of overland wires would necessitate such frequent changes of the balancing resistances as to render efficient working the exception rather than the rule. But it is found practically that, instead of an

exact balance being always required, the signalling can be continued for great lengths of time without any change of adjustment; and, moreover, the balancing resistances may sometimes be altered very considerably without actually interrupting the signalling. The actual received current may be considered as the algebraical sum of two parts—one the proper received current, the other an interfering current produced by inexact balance. In the double-current Morse system in common use in England the marks are made by one current (say, positive), and the spaces by the negative current. If C is the strength of the received current, then the whole range of the current is $2C$. In the single-current Morse system employed on the Continent and elsewhere there is no current during the spaces; hence the range of the current in the receiving instrument is only C . Therefore an instrument that admits of being worked either by single or double currents, as magnetized or polarized relays, will give signals twice as strong with double currents as with single with the same battery-power. This applies both to ordinary single working and to duplex working. In the latter there is a further advantage in favour of double currents. It is theoretically possible to work duplex with double currents when the interfering currents are little less strong than the received currents; for as the received current is always either $+C$ or $-C$, the superposition of any current of less strength than C will not alter the sign of C , whether $+$ or $-$. On the other hand, in single-current working the received current is always either C or zero. In the first case the current C overpowers the tension of a spring or other opposing force; and in the latter the spring is unopposed. The most rapid signalling is to be obtained when the forces moving the armature or tongue of the relay are equal in each direction; and then the retractile force of the spring must be equivalent to a reverse current of the strength $\frac{1}{2}C$. Therefore the interfering currents in duplex working with single currents must never be so great as $\frac{1}{2}C$ —thus giving an immense advantage to the double-current system as regards freedom from interruption by inexact balance or other causes, in addition to the advantage before mentioned of giving signals of twice the strength. It is found by experiment that duplex working (Morse) will not be actually interrupted until the interfering currents are as much as $\frac{1}{3}$ or $\frac{1}{2}$ the strength of the received currents with double currents, and $\frac{1}{6}$ or $\frac{1}{4}$ with single currents—although no hard and fast line can be drawn, owing to the very numerous causes in operation. On an overland wire worked duplex with differential relays and double currents the resistance which gave exact balance was, at one end, 2560

ohms, which could be increased to 3860 or diminished to 1760 ohms without interrupting the working. At the other end the balancing resistance could be varied from 3000 to 6000 ohms without interfering with rapid signalling. The variation allowable above balance is always much greater than below, because the interfering current is inversely proportional to the resistance external to the battery, which is increased when the balancing resistance is increased. In the above example the line was fairly insulated. When the insulation falls, the effect is to strengthen the sent and weaken the received currents; consequently the interfering currents bear a larger ratio to the received currents for a given change of balance; and the balance therefore requires nicer adjustment. The extreme case is reached with the very low insulation which prevails in this country in continuous wet weather, when not much difference can be detected between the resistance of the wire whether it is insulated or to earth at one end. Under such circumstances a very small change of balance is sufficient to upset the working. The ratio of the interfering to the received currents may be diminished *ad lib.* by increasing the resistance of the apparatus, or more simply by inserting a constant resistance in the main circuit. As, however, it is only when the insulation is very bad and the received current very weak that the interfering current due to inexact balance attains such a proportional strength as to mutilate the signals, the increase in the resistance of the apparatus would be an evil rather than a benefit, on account of the reduction in the strength of the received signals, already very weak, that would ensue.

The two other systems described in my former paper (Phil. Mag. June 1873) are not likely, in accordance with the principle of the survival of the fittest, to come into practical use; and it is therefore unnecessary to enter into details concerning them. But this I may observe, that in both of them the arrangement which produces the strongest signals at both stations is also the most sensitive balance. That this should be the case in four different systems, renders it probable that it is universally true for all duplex systems, in which some kind of balance is concerned.

VI. *On the Representation of an Uneven Number as a Sum of four Squares, and as the Sum of a Square and two Triangular Numbers.* By J. W. L. GLAISHER, M.A., F.R.S.*

I. **A**T the end of a paper entitled "Verification of an Elliptic Transcendent Identity," published in the *Philosophical Magazine* for June 1874, I have reproduced in an expanded form a proof (due to Gauss, but the steps of which he had only briefly indicated) of the identity

$$(1 + 2q + 2q^4 + 2q^9 + \&c.)^4 = (1 - 2q + 2q^4 - 2q^9 + \&c.)^4 \\ + (2q^{\frac{1}{4}} + 2q^{\frac{9}{4}} + 2q^{\frac{25}{4}} + \&c.)^4;$$

and it occurred to me subsequently that it would be interesting to demonstrate the truth of the identity by showing, directly, from arithmetical considerations, the equality of the coefficients of like powers of x on the two sides of the equation.

The coefficients of the even powers of x are obviously the same; and the theorem to be proved is that, N being any uneven number, the number of representations† of $4N$ as the sum of four uneven squares is equal to twice the number of representations of N as the sum of four, or a less number of, uneven squares. It is convenient in what follows to have a name for a decomposition of a number as a sum of four squares, irrespective of the order in which they are written; and I shall call such a decomposition a resolution; so that $a^2 + b^2 + c^2 + d^2$, $a^2 + c^2 + b^2 + d^2$, &c. are all the same resolution.

Suppose $N = a^2 + b^2 + c^2 + d^2$ is a resolution of N , then we can derive from it two resolutions of $4N$ into uneven squares, viz.

$$\begin{array}{ll} \text{I.} & (a+b+c+d)^2, \quad \text{or II.} \quad (a+b+c-d)^2, \\ & + (a+b-c-d)^2, \quad \quad \quad + (a+b-c+d)^2, \\ & + (a-b-c+d)^2, \quad \quad \quad + (a-b+c+d)^2, \\ & + (-a+b-c+d)^2, \quad \quad \quad + (-a+b+c+d)^2; \end{array}$$

and it is easily seen that no other set of combinations formed by addition and subtraction of the elements a, b, c, d will give a resolution not included in these two.

If any one of the elements, say $a, = 0$, then I. and II. be-

* Communicated by the Author.

† "In counting the number of compositions by addition of squares, two compositions are to be considered as different if, and only if, the same places in each are not occupied by the same squares; but in counting the number of representations we have to attend also to the signs of the roots of the squares. Thus each composition by the addition of four squares, none of which is zero, is equivalent to sixteen representations."—Professor H. J. S. Smith, "Report on the Theory of Numbers," art. 127 (British Association Report, Birmingham Meeting, 1865, p. 337).

come identical ; if also another, $b, = 0$, the resolution is of the form $P^2 + P^2 + Q^2 + Q^2$; if further $c = 0$, the form is

$$d^2 + d^2 + d^2 + d^2.$$

Confining our attention first to the case in which no one of the quantities a, b, c, d is zero, we see that every resolution of N gives rise to two resolutions of $4N$:—one of class I., in which the largest square is $(a + b + c + d)^2$; and the other formed from it by changing the sign of any one letter throughout. Consider any two resolutions of N , viz. $a^2 + b^2 + c^2 + d^2$ and $\alpha^2 + \beta^2 + \gamma^2 + \delta^2$; they give rise to four resolutions of $4N$, viz. two of class I. and two of class II. ; and it is easily seen that the two resolutions of class I. cannot be identical (and also that those of class II. cannot be identical), unless $a^2, b^2, c^2, d^2 = \alpha^2, \beta^2, \gamma^2, \delta^2$ *, which is supposed not to be the case. But it requires further examination to see that one of the resolutions of class I. cannot be equal to one of those of class II. ; viz. we have to show that, if $\alpha^2 + \beta^2 + \gamma^2 + \delta^2$ be an uneven number, and if

$$\begin{aligned} & (\alpha + \beta + \gamma - \delta)^2 + (\alpha + \beta - \gamma + \delta)^2 + (\alpha - \beta + \gamma + \delta)^2 \\ & \qquad \qquad \qquad + (-\alpha + \beta + \gamma + \delta)^2 \\ = & (a + b + c + d)^2 + (a + b - c - d)^2 + (a - b - c + d)^2 \\ & \qquad \qquad \qquad + (-a + b - c + d)^2, \end{aligned}$$

term for term, then it follows that $\alpha^2, \beta^2, \gamma^2, \delta^2 = a^2, b^2, c^2, d^2$.

The conditions give

$$\begin{aligned} \alpha + \beta + \gamma - \delta &= \pm (a + b + c + d), \\ \alpha + \beta - \gamma + \delta &= \pm (a + b - c - d), \\ \alpha - \beta + \gamma + \delta &= \pm (a - b - c + d), \\ -\alpha + \beta + \gamma + \delta &= \pm (-a + b - c + d); \end{aligned}$$

and a few moments' consideration shows that the ambiguities on the right-hand side must be all replaced by + or by -, or that two must be + and two -. (For, *ex. gr.*, take the first three + and the last -, and we have by addition $\alpha + \beta + \gamma + \delta = 2a$; that is, an uneven number $= 2a$.) Replacing all the ambiguities by the same (say the +) sign, the equations are

$$\begin{aligned} \alpha + \beta + \gamma - \delta &= a + b + c + d, \\ \alpha + \beta - \gamma + \delta &= a + b - c - d, \\ \alpha - \beta + \gamma + \delta &= a - b - c + d, \\ -\alpha + \beta + \gamma + \delta &= -a + b - c + d, \end{aligned}$$

* By $a^2, b^2, c^2, d^2 = \alpha^2, \beta^2, \gamma^2, \delta^2$ is meant that the four former squares are to be equal to the latter independently of order ; for example, $a^2 = \beta^2, b^2 = \alpha^2, c^2 = \delta^2, d^2 = \gamma^2$ satisfies the equation.

so that $\alpha=a$, $\beta=b$, $\gamma=d$, $\delta=-c$; and, generally, we must have $\alpha^2, \beta^2, \gamma^2, \delta^2 = a^2, b^2, c^2, d^2$. The second case in effect includes the other (in which the resolutions are of the same class) by supposing the sign of δ changed.

The presence of zero values of a, b , &c. in no way affects the argument (except by reducing to equivalence the two classes I. and II.); so that generally we see that every resolution of N gives rise to two resolutions of $4N$, unless a zero value occurs, when there is but one. It is also evident that the transformed resolutions are of the same form as regards equality of squares as the original resolutions: viz. if a^2, b^2, c^2, d^2 are all different, the squares in the transformations are all different; if $a^2=b^2$, two squares in each of the transformations are equal, and so on. This is true also if one of the squares be zero. If two be zero, the transformation converts c^2+d^2 into $P^2+P^2+Q^2+Q^2$; and if three be equal, into $d^2+d^2+d^2+d^2$ as before mentioned.

We have now to show that every resolution of $4N$ as the sum of four uneven squares A^2, B^2, C^2, D^2 , may be derived from a resolution of N of the form $\alpha^2+\beta^2+\gamma^2+\delta^2$ by a transformation of class I. or class II.; that is, that we can always find integer values of $\alpha, \beta, \gamma, \delta$ that satisfy the system of equations

$$\begin{aligned}\alpha+\beta+\gamma-\delta &= \pm A, \\ \alpha+\beta-\gamma+\delta &= \pm B, \\ \alpha-\beta+\gamma+\delta &= \pm C, \\ -\alpha+\beta+\gamma+\delta &= \pm D.\end{aligned}$$

Taking the positive signs throughout for the first series of values, and changing the sign of D for the second,

$$\begin{aligned}\alpha &= \frac{1}{4}(A+B+C-D) \text{ or } = \frac{1}{4}(A+B+C+D), \\ \beta &= \frac{1}{4}(A+B-C+D) \quad ,, \quad \frac{1}{4}(A+B-C-D), \\ \gamma &= \frac{1}{4}(A-B+C+D) \quad ,, \quad \frac{1}{4}(A-B+C-D), \\ \delta &= \frac{1}{4}(-A+B+C+D) \quad ,, \quad \frac{1}{4}(-A+B+C-D);\end{aligned}$$

and, since D is uneven, one (and only one) of these systems gives integer values to $\alpha, \beta, \gamma, \delta$. Thus there is no resolution of $4N$ into uneven squares that is not derivable by transformation from a resolution of N ; and it only remains to connect the representations of N with those of $4N$.

1^o. Consider the resolutions in which no one of the squares a^2, b^2, c^2, d^2 is equal to zero; then, since each resolution of N gives rise by transformation to resolutions of the same form as regards equality of squares, each resolution of both N and $4N$ produces the same number of compositions, and therefore also of representations; so that the number of representations of

$4N$ as a sum of uneven squares is double the number of representations of N . 2°. If one of the squares is zero, no alteration is made in the number of compositions; but each composition of N gives rise to 8 representations only instead of 16, and the two classes become identical; so that in this case also the number of representations of $4N$ is double the number of representations of N . 3°. If two squares are zero (so that the other two must be different), the number of representations produced by each resolution of N is 12.4, while the corresponding resolution of $4N$ (being of the form $P^2 + P^2 + Q^2 + Q^2$) gives rise to 6.16; and, 4°, if N is an uneven square, this resolution gives 8 representations, while the corresponding resolution $d^2 + d^2 + d^2 + d^2$ of $4N$ gives 16 representations. Thus, universally the number of representations of $4N$ as the sum of four uneven squares is equal to double the number of representations of N as a sum of four, or a less number of, squares.

I here add an example of the transformation. Take $N = 117^*$, then

$$\begin{aligned}
 8^2 + 6^2 + 4^2 + 1^2 & \text{ gives } 4N = 19^2 + 9^2 + 5^2 + 1^2 \dots (I.) \\
 & = 17^2 + 11^2 + 7^2 + 3^2, \dots (II.) \\
 9^2 + 4^2 + 4^2 + 2^2 & \text{ ,, } 4N = 19^2 + 7^2 + 7^2 + 3^2 \dots (I.) \\
 & = 15^2 + 11^2 + 11^2 + 1^2 \dots (II.) \\
 10^2 + 3^2 + 2^2 + 2^2 & \text{ ,, } 4N = 17^2 + 9^2 + 7^2 + 7^2 \dots (I.) \\
 & = 13^2 + 13^2 + 11^2 + 3^2 \dots (II.) \\
 7^2 + 6^2 + 4^2 + 4^2 & \text{ ,, } 4N = 21^2 + 5^2 + 1^2 + 1^2 \dots (I.) \\
 & = 13^2 + 13^2 + 9^2 + 7^2, \dots (II.) \\
 6^2 + 6^2 + 6^2 + 3^2 & \text{ ,, } 4N = 21^2 + 3^2 + 3^2 + 3^2 \dots (I.) \\
 & = 15^2 + 9^2 + 9^2 + 9^2, \dots (II.) \\
 10^2 + 4^2 + 1^2 & \text{ ,, } 4N = 15^2 + 13^2 + 7^2 + 5^2, \\
 8^2 + 7^2 + 2^2 & \text{ ,, } 4N = 17^2 + 13^2 + 3^2 + 1^2, \\
 9^2 + 6^2 & \text{ ,, } 4N = 15^2 + 15^2 + 3^2 + 3^2.
 \end{aligned}$$

Apart from the verification of the elliptic transcendent identity, the connexion between the separate resolutions of N and the corresponding resolutions of $4N$ is interesting.

II. The following investigation relates to the representation of an uneven number as the sum of one square and two triangular numbers.

* The number of representations of an uneven number as the sum of four squares is eight times the sum of its divisors, and 117 is chosen on account of the number of its divisors.

Consider the identity ($N=$)

$$\begin{aligned}
 m^2 + \frac{p \cdot p + 1}{2} + \frac{q \cdot q + 1}{2} \\
 &= \left(\frac{p-q}{2} \right) + \frac{1}{2} \left(m + \frac{p+q}{2} \right) \left(m + \frac{p+q}{2} + 1 \right) \\
 &\quad + \frac{1}{2} \left(-m + \frac{p+q}{2} \right) \left(-m + \frac{p+q}{2} + 1 \right) \\
 &= \left(\frac{p+q+1}{2} \right)^2 + \frac{1}{2} \left(m + \frac{p-q-1}{2} \right) \left(m + \frac{p-q+1}{2} \right) \\
 &\quad + \frac{1}{2} \left(-m + \frac{p-q-1}{2} \right) \left(-m + \frac{p-q+1}{2} \right),
 \end{aligned}$$

and observe that, if p be of the form $4n+3$ or $4n$, $\frac{1}{2}p \cdot p + 1$ is even, and that, if p be of the form $4n+1$ or $4n+2$, it is uneven. Then (N being uneven) if m be uneven, and consequently $\frac{1}{2}p \cdot p + 1$ and $\frac{1}{2}q \cdot q + 1$ be both even or both uneven, then, according as p and q are both of the same form or of different forms, the first or second transformation gives rise to the sum of an even square, an even triangular number, and an uneven triangular number. And, conversely, an even square and two triangular numbers, one even and one uneven, are transformed into an uneven square and two triangular numbers, both even or both uneven. This is evident on examining the different cases. We thus have the curious theorem that to every representation of an uneven number as an even square and two triangular numbers, there corresponds a representation as an uneven square and two triangular numbers, and *vice versâ*; and we further see that the presence of a zero square corresponds to a case of equality of the two triangular numbers*. If, therefore, all the representations of an uneven number as the sum of a square and two triangular numbers be written down, in half the number the square is even and in half uneven. Stated analytically, the theorem is

$$\begin{aligned}
 (1 + 2q^4 + 2q^{16} + \&c.)(1 + q^6 + q^{10} + \&c.)(q + q^3 + q^{15} + \&c.) \\
 &= (q + q^9 + q^{25} + \&c.)(1 + q^6 + q^{10} + \&c.)^2 \\
 &\quad + (q + q^9 + q^{25} + \&c.)(q + q^3 + q^{15} + \&c.)^2,
 \end{aligned}$$

* Of course 0 is to be treated as a square, and also as an even triangular number whose root (calling n the root of $\frac{1}{2}n \cdot n + 1$) is 0; and the square numbers that are also triangular are to be treated both as squares and triangular numbers. As an example of the theorem, take $N=31$, and $25+6+0$, $25+3+3$, $9+21+1$, $1+15+15$ transform into $4+21+6$, $0+28+3$, $16+15+0$, $0+21+10$ respectively (writing the square first in each partitionment).

which is readily transformed into

$$\left(\frac{1+q+q^3+q^6+q^{10}+\&c.}{1-q-q^3+q^6+q^{10}-\&c.} \right)^2 = \frac{1+2q+2q^4+2q^9+2q^{16}+\&c.}{1-2q+2q^4-2q^9+2q^{16}-\&c.},$$

an identity derivable at once from elliptic-function formulæ. The above arithmetical process thus affords a proof of this identity.

VII. On Salt Solutions and Attached Water.

By FREDERICK GUTHRIE*.

[Continued from S. 4. vol. xlix. p. 276.]

III.

A few special Cryogens and Cryohydrates.

I HAVE, in the first place, to redeem the promise given in § 89, and, by discussing the behaviour as cryogens and cryohydrates of a few peculiar salts, to complete one part of my undertaking.

§ 105. *Chloride of Calcium as a Cryogen.* A. *Crystallized* $\text{CaCl}_2 + 3\text{H}_2\text{O}$.—The heat liberated when anhydrous CaCl_2 is brought into contact with water interferes with, by diminishing, the manifestation of cold due to the proper liquefaction of the salt and of the ice when the two are brought together. Accordingly in experiments with chloride of calcium as a cryogen which are to serve as a guide to the temperature at which the formation of the cryohydrate may be predicted, we may conveniently begin with the crystallized salt. A seasonable fall of snow last winter enabled me to extend my experiments with the chloride of calcium, not only in regard to the relative quantity of the two constituents, but to the relative effects of snow and pounded ice, and to establish conclusively that the two are of quite equal power when used as one element of a cryogen. The crystallized $\text{CaCl}_2 + 3\text{H}_2\text{O}$ was finely powdered and placed in weighed quantities on the weighed snow or crushed ice in a beaker, the two being stirred continuously with the thermometer. Under these conditions the lowest temperature is quickly reached.

* Communicated to the Physical Society, June 12, 1875.

TABLE XII.—Column (1) the weight in grammes of the powdered $\text{CaCl}_2 + 3\text{H}_2\text{O}$, column (2) the weight in grammes of the powdered ice or snow, columns (3) and (4) the respective percentages of the two, column (5) the temperature.

(1) $\text{CaCl}_2 + 3\text{H}_2\text{O}$, grammes.	(2) Snow or ice, grammes.	(3) Per cent., $\text{CaCl}_2 + 3\text{H}_2\text{O}$	(4) Per cent., snow or ice.	(5) Temperature, Centigrade.
* 100	33 snow	75	25	-21°
100	50 snow	66.6	33.3	-28
94	60 ice	61.5	38.5	-33
* { 75	50 snow	60	40	-32
* { 75	50 ice	60	40	-32
* { 50	75 snow	40	60	-27
* { 50	100 snow	33.3	66.6	-24
* { 50	100 ice	33.3	66.6	-24.5

From this, especially from the examples marked *, it appears that snow and ice are of equal value as elements of cryogenes, and that the greatest cold is obtained on mixing 38.5 of ice or snow with 61.5 of the crystallized salt. This is the ratio between $\text{CaCl}_2 + 3\text{H}_2\text{O}$ and $5.74\text{H}_2\text{O}$, or $4(\text{CaCl}_2 + 3\text{H}_2\text{O})$ and $23\text{H}_2\text{O}$ or CaCl_2 and $8.74\text{H}_2\text{O}$.

§ 106. B. *Anhydrous* CaCl_2 as *Cryogen*.—Freshly fused CaCl_2 was finely powdered and mixed with finely crushed ice.

TABLE XIII.—Columns (1) and (2) show the actual quantities, in grammes, taken of CaCl_2 and ice respectively; columns (3) and (4) show the percentages; column (5) shows the lowest temperature reached; and the numbers in column (6) show the temperature to which the mixture rose after all the ice was melted in those cases where the proportion of ice was least.

(1) CaCl_2 , grammes.	(2) Ice, grammes.	(3) CaCl_2 , per cent.	(4) Ice, per cent.	(5) Lowest temp. Cent.	(6) Rose to temp. Cent.
50	50	50.0	50.0	+33	
50	75	40.0	60.0	+10	to +18
50	90	35.7	64.3	+4	" +14
50	95	34.5	65.5	+3	" +11
50	100	33.3	66.7	-2	" +9
50	125	28.6	71.4	-10	
33.3	100	25.0	75.0	-13	
25	100	20.0	80.0	-15	
20	100	16.7	83.3	-14.5	
16.6	100	14.2	85.8	-12	
20	140	12.5	87.5	-12	

Here the lowest temperature is reached at the ratio 1 : 4, or about $\text{CaCl}_2 + 15 \text{H}_2\text{O}$. We must not attach too much importance to the numbers of this Table, because, as before remarked, the liberation of heat when the anhydrous salt dissolves in, or rather combines with water, largely supplies heat for the liquefaction of the solid ice and salt. And accordingly, though the heat-quantity concerned may be deduced from knowledge of the quantity due to such combination and the specific heat of the solution and the heat of liquefaction of the ice, yet the temperature or heat-tension, being a function of the time or rate of liquefaction, cannot be so deduced. The caking of the chloride is also a source of experimental difficulty.

§ 107. *Monohydrated Chloride of Calcium as a Cryogen.*—On baking the terhydrate of chloride of calcium a porous mass is left, which is used for the absorption of water in gases. This, though called anhydrous in commerce, contains about one molecule of water. And the presence of the water is betrayed when the body is used as a cryogen. The chloride, finely powdered, was weighed upon snow or ice and stirred with the thermometer unceasingly. The higher ratios of frozen water were ice, the lower ones were snow; the two are connected together at the 50 per cent. ratios, marked *.

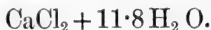
TABLE XIV.

$\text{CaCl}_2 + \text{H}_2\text{O}$, grammes.	Snow or ice, grammes.	$\text{CaCl}_2 \cdot \text{H}_2\text{O}$, per cent.	Snow or ice, per cent.	Temperature, Centigrade.
100	33 snow	75	25	+ 5
100	50 "	66·7	33·3	— 4
75	50 "	60	40	— 10
*50	50 "	50	50	— 17
*50	50 ice	50	50	— 17
50	100 "	33·3	66·7	— 17
50	150 "	25	75	— 16
50	200 "	20	80	— 16
50	300 "	14·3	85·7	— 16
50	400 "	11·1	88·9	— 12
50	600 "	7·7	92·3	— 10

The margin of ratio for minimum temperature is considerable. The minimum temperature itself is intermediate between the minima for CaCl_2 and $\text{CaCl}_2 + 3 \text{H}_2\text{O}$.

§ 108. *The Cryohydrate of Chloride of Calcium.*—A solution of $\text{CaCl}_2 \cdot 3 \text{H}_2\text{O}$, saturated at the temperature of the air (10°C .), was cooled to -20° in snow and ice. The mother-liquor was then exposed to the carbonic-acid-and-ether cryogen. The temperature sank to -37°C . under continual separation of a transparent hydrate. At -37°C . an opaque cryohydrate is

formed, and the temperature remains constant for a considerable time. The opaque crystals, on being remelted, showed the following composition. By a silver-determination 6.6835 contained 2.3365 of CaCl_2 . This shows 36.45 of CaCl_2 , and exhibits the molecular ratio



Mixed Salts, as Cryogens and Cryohydrates.

§ 109. Still bearing in mind the problem of sea-water, we may next consider the behaviour of mixtures of soluble salts, both as cryogens and cryohydrates. Even regarding salts as preserving the individuality of their metallic and non-metallic parts when mixed together, there is a variety of recombination possible. Thus taking the typical salts AX and BY, we may have $\text{A}=\text{B}$ or $\text{X}=\text{Y}$. No double recombination is possible in either of the latter cases. But there may still be formed double salts in which each metal is engaged with both non-metallic constituents. I suppose that in the present state of chemical knowledge it would be unwise to assert that any double salt is impossible, especially after the evidence which has been so abundantly given above that the body water is virtually pantameric.

On the other hand, remembering the liberal margin of ratio between ice and a salt for the production of the normal or maximum cold, we shall get by examining the temperature of the cryogen of a mixture of salts and the temperature of their common cryohydrate, if they have one, a valuable and indeed unique insight into their molecular relation, in the sense of determining whether double salts or double recombination ensues. At present I have confined my examination to the various cases of two salts. Of these a few typical examples are taken; and in the first place a pair of salts are examined which differ only in their metals.

§ 110. *Mixed Nitrates of Potassium and Sodium as Cryohydrate.*—The salts were just fused, poured on a slab, crushed while hot, and bottled. The two salts were weighed in monomolecular ratio; namely, of

KNO_3 there were 33.666 grms.
 NaNO_3 „ 28.333 „

These quantities were mixed, completely dissolved in water, and allowed to evaporate at 13°C . till crystallization began. The mixture was then cooled in an ice-salt cryogen. At about $+12^\circ$ to $+10^\circ \text{C}$. transparent crystals resembling KNO_3 began to form, and continued to do so as the temperature fell. At about -7° an opaque cryohydrate appeared. This went

on till -10° C. was reached, when the whole was pasty. At -13° the mass was not yet dry. At $-14^{\circ}5$ it was nearly dry. At $-16^{\circ}5$ to -17° it became perfectly dry. We find then that

The cryohydrate of KNO_3	solidifies at	-2.7
" "	NaNO_3	" -17.5
" "	equiv. mixture	" -7 to -17° .

Hence it appears that the presence of the nitrate of sodium lowers the temperature at which the cryohydrate of the nitrate of potassium is formed, while the temperature of final solidification is virtually as low as when the nitrate of sodium was alone present.

§ 111. *Mixed Nitrates of Potassium and Sodium as Cryogen.*—The powdered nitrates being mixed in the same proportion as in § 110, namely 11.22 grms. of KNO_3 and 9.44 grms. of NaNO_3 , and stirred with about 80 grains of crushed ice, gave a temperature of $-16^{\circ}8$. This is clearly again due to the more powerful cryogen NaNO_3 , which (§ 75) gave $-16^{\circ}5$.

§ 112. *Mixed Chlorides of Potassium and Sodium as Cryohydrate and Cryogen.*—Taking the salts in monomolecular ratio, namely of

KCl	taking	18.625
NaCl	"	14.625

dissolving in water and allowing it to stand until crystallization just began, a solution was obtained which solidified completely at -21° C. On mixing the mixture of the same salts in the same proportion with crushed ice, the resulting temperature was $-21^{\circ}8$. The temperatures in both cases are evidently governed by the action of the NaCl; for

The cryohydrate of KCl	solidifies at	-11.4
" "	NaCl	" -22
" "	equiv. mixture	" -21
Temperature of KCl	as cryogen	-10.5
"	NaCl	" -22
"	equiv. mixture	" -21

There is so much difficulty attending the accurate separation of sodium and potassium that no method of analysis of them with which I am acquainted is free from the possible error of 2 or 3 per cent. upon the quantity. Accordingly, for the quantitative study of the mixed cryohydrates or cryohydrate of the mixed salts, the following two simple ones were chosen.

§ 113. *Mixed Chlorides of Potassium and Ammonium as Cryohydrates and Cryogens.*

Of NH_4Cl	were taken	13.375 grms.
" KCl	"	18.625 "

These were dissolved, mixed, and allowed to evaporate till crystallization just began. On cooling, solidification did not begin till $-16^{\circ}5$ or -17° . The temperature remained for a long time at -17° ; and when it had reached $-18^{\circ}8$ the whole mass was solid. Two analyses of the remelted last crop of cryohydrate were made. The solution was weighed into a covered Bohemian-glass basin, evaporated at 100° C. to dryness, kept *in vacuo* over sulphuric acid, weighed, gently ignited, and reweighed.

	grms.	grm.	grm.
(a) 4.035 of solution gave	0.9505	of total residue and	0.4890 KCl
(b) 5.817	1.3725	„ „	0.7005 KCl

So that the percentage compositions were:—

	a.	b.	Mean.
KCl . .	12.12	12.04	12.08
NH ₄ Cl .	11.43	11.55	11.49
H ₂ O .	76.45	76.41	76.43

The molecular ratios are accordingly

KCl = 0.1608, NH₄ Cl = 0.2148, and H₂ O = 4.246,

or

KCl + 1.33 NH₄ Cl + 26.4 H₂ O,

or

3 KCl + 4 NH₄ Cl + 79.2 H₂ O.

Since the water-worth of NH₄ Cl is 12.4 and that of KCl is 16.61 (§ 88), we cannot look upon the compound body as a mixture of the two cryohydrates; but we may perhaps regard it as consisting of seven molecules of the cryohydrate of chloride of ammonium in which three molecules of ammonium are replaced by potassium.

The possible precision in the analysis of the mixed chlorides of potassium and ammonium induced me to extend the examination to the intermediate crops of cryohydrates. Accordingly, the dry salts being mixed as before in equivalent ratio, dissolved, concentrated and cooled, five successive crops of cryohydrate were collected as soon as the temperature had sunk to $-16^{\circ}5$, the last being the mother-liquor.

	(1)	(2)	(3)	(4)	(5)	(6)
Solution.....	6.6090	5.8765	4.0540	6.8210	6.4720	5.0750
Total residue ..	1.4810	1.3445	0.9375	1.5720	1.4960	1.2145
KCl7870	0.6760	0.4630	0.7815	0.7410	0.5890

Therefore

KCl:	11.90	11.50	11.54	11.45	11.44	11.60
NH ₄ Cl	10.50	11.37	11.58	11.59	11.67	12.32
H ₂ O	77.60	77.23	76.88	76.96	76.89	76.08

Hence it appears that the composition of the successive crops of cryohydrate is nearly unchanged. The later the crop, the poorer is it in water and potassium and the richer in ammonium. And the increase in the ammonium is greater than the decrease in the potassium. The same ratio of the same chlorides, when used as a cryogen, gave -18° . Accordingly

The cryohydrate of KCl solidifies at $-11\frac{1}{4}$.

” ” NH_4Cl ” -15

” ” equiv. mixture ” -17

and

As a cryogen KCl gives $-10\cdot5$

” NH_4Cl ” -16

” equiv. mixture ” -18

§ 114. *Mixed Nitrates of Barium and Strontium as Cryohydrates and Cryogens.*

7·05 grms. of $\text{Sr } 2\text{NO}_3$,

and

8·70 grms. of $\text{Ba } 2\text{NO}_3$

were dissolved, mixed, and allowed to evaporate to incipient crystallization. At $-1^{\circ}\cdot2$ a cryohydrate began to form; and the whole was solid at $-4^{\circ}\cdot3$. Employed as a cryogen, the same mixture of salts gave $-5^{\circ}\cdot8$. We have therefore

The cryohydrate of $\text{Sr } 2\text{NO}_3$ solidifies at $-\overset{\circ}{6}$

” ” $\text{Br } 2\text{NO}_3$ ” $-\overset{\circ}{0}\cdot8$

” ” equiv. mixture solidifies at $-4\cdot3$

and

As a cryogen $\text{Sr } 2\text{NO}_3$ gives $-\overset{\circ}{6}$

” $\text{Ba } 2\text{NO}_3$ ” $-\overset{\circ}{0}\cdot9$

” equiv. mixture gives $-5\cdot8$

§ 115. *Mixed Chlorides of Barium and Strontium as Cryohydrates and Cryogens.*

6·933 grms. of BaCl_2

and

5·283 grms. of Sr Cl_2 ,

both salts having been gently ignited, were dissolved, mixed, and evaporated to saturation. On cooling, a large quantity of transparent salt fell out above 0° . At about -10° a cryohydrate began to form, and the whole was nearly solid at $-13^{\circ}\cdot5$ to -14° . A small portion, however, remained liquid to -18° .

Mixed with snow, the above salts in the above proportion

lowers the temperature to $-16^{\circ}7$. Comparing as before,

The cryohydrate of Sr Cl_2 solidifies at	. . .	-17°
„ „ BaCl_2 „	. . .	-8
„ „ equiv. mixture solidifies at		-18

and

As a cryogen, SrCl_2 gives	. . .	-17°
„ BaCl_2 „	. . .	$-7\cdot2$
„ equiv. mixture gives		$-16\cdot7$

As it is almost impossible to separate barium and strontium with very great precision, and as it was highly desirable to compare the relative quantities in the cryohydrate in at least one other case besides that of § 113, I chose in the right place the—

§ 116. *Mixed Sulphates of Sodium and Ammonium as Cryohydrates and Cryogens.*

6·6 grms. of $(\text{NH}_4)_2 \text{SO}_4$

and

7·1 grms. of $\text{Na}_2 \text{SO}_4$

were dissolved, mixed, and allowed to evaporate to saturation. On cooling to 0° , acicular crystals were deposited (of sulphate of sodium). At $-4^{\circ}5$ a cryohydrate began to form; the whole was solid and dry at -7° . On analysis, by evaporation to dryness and subsequent strong heating, the portion last to solidify was found in two specimens to have the following composition:—

	a.	b.	Mean.
$(\text{NH}_4)_2 \text{SO}_4$. .	12·23	12·25	12·24
$\text{Na}_2 \text{SO}_4$. .	4·82	4·86	4·84
$\text{H}_2 \text{O}$. . .	82·95	82·89	82·92

The molecular ratios are accordingly

$\text{Na}_2 \text{SO}_4$	1
$(\text{NH}_4)_2 \text{SO}_4$	2·72
$\text{H}_2 \text{O}$	135·2

or nearly



As (§ 88) the water-worth of $\text{Na}_2 \text{SO}_4$ is 165·6, and that of $(\text{NH}_4)_2 \text{SO}_4$ is 10·2, this cryohydrate can neither be regarded as a substitutive cryohydrate like that suggested in § 113 for the cryohydrate of the chlorides of potassium and ammonium, nor as a simply additive cryohydrate; for the joint water-worth is less than that of the ammonium salt alone. The double sulphate of sodium and ammonium is formed; and this has the specific water-worth of 135·2 or (taking the higher molecule) of 541.

That the two salts are not independent of one another, appears from their joint action as a cryogen; for mixed in the above equivalent ratio and stirred with snow, they give -16° . Comparing the joint with the separate effects,

The cryohydrate of $(\text{NH}_4)_2\text{SO}_4$	solidifies at	-17°
„ „ Na_2SO_4	„ „	-0.7
„ „ equiv. mixture	solidifies at	-7

and

As a cryogen, $(\text{NH}_4)_2\text{SO}_4$	gives .	-17.5°
„ Na_2SO_4	„ .	-0.7
„ equiv. mixture	gives	-16

It is manifest that the doubling of the salt is not taking place during the solution of its two constituents when their mixture is a cryogen, but that it takes place when from solution they unite together in partnership with water as a cryohydrate.

§ 117. *Variation of both Constituents (AX + BY).*—The fascinating hypothesis of Berthollet that double decomposition always partially ensues when two such salts as AX and BY are mixed, even when no removal from the chemical field takes place of any of the possible new bodies resulting from the recombination, may be critically approached from the side of the cryohydrates with great profit. Let me put the question in two concrete forms, and indeed in two of the forms which I immediately propose to examine experimentally.

Is a solution consisting of

202 grms. of KNO_3 and 142 grms. of Na_2SO_4

dissolved in water, identical with a solution consisting of

170 grms. of NaNO_3 and 174 of K_2SO_4

dissolved in the same amount of water?

If they are identical, does any one of the salts KNO_3 , NaNO_3 , K_2SO_4 , Na_2SO_4 exist as such in either solution?

§ 118. *Comparison between a mixture of 2NaNO_3 and K_2SO_4 and a mixture of 2KNO_3 and Na_2SO_4 as Cryohydrates and Cryogens.*

33.666 grms. of KNO_3

and

23.666 grms. of Na_2SO_4 ,

both anhydrous, were mixed, dissolved, and evaporated to saturation. This gave solution (1). Again,

28.333 grms. of NaNO_3

and

29 grms. of K_2SO_4

were treated in the same way. This gave solution (2). On cooling (1) and (2), they showed the same behaviour throughout and solidified to dryness at -5° . And accordingly there is so far nothing in their behaviour to disprove their identity. But this does not show that partial double decomposition has ensued in both cases to produce in each mixture a portion of the original synthetical constituents of the other. On the contrary, a comparison of this experiment with those above discussed (of the form $AX + BX$) goes very far to prove, not that there are four salts in each of the solutions 1 and 2, but that there is none of the original salts in either. For it has appeared that when AX and BX are mixed in solution, the temperature of final solidification is in no case sensibly above the temperature required for the solidification of that constituent whose solidification takes place at the lowest temperature. Now, if in solution (1) or (2) there were nitrate of sodium present, whether original or derived, the final solidification would not take place above -17° instead of -5° as found. The absence of nitrate of sodium argues forcibly for the absence of each of the other single salts. The most rational formula for such a mixture is the most empirical one possible. As we cannot intertwine the symbols into a monogram in three dimensions, I arrange them alphabetically, and denote either of the solutions 1 or 2 by $H_{2n}KN_2NaO_{n+10}S$. I suppose the time will come when it will be held as illogical to consider that KNO_3 exists in a solution of nitre as it would be to regard a circle as a straight line which has been bent round.

§ 119. The same salts as in § 118 were now taken, but not in equivalent quantity; namely

		grms.		grms.
For solution A there were	66 of Na_2SO_4	and	87.72 of KNO_3 ,	
B ,,	91 ,, K_2SO_4	,,	85 ,, $NaNO_3$.	

The salts, as before, were mixed, dissolved, evaporated to saturation, and cooled. The solutions being saturated at $+12^{\circ}$, were first cooled in ice for an hour. During this, two kinds of crystals separated from each in sensible quantity. On further cooling, the temperature became and remained constant at -5° up to solidification. The last liquid portions of A and B having been removed to separate vessels before final solidification, were examined as far as their proportion of sulphuric acid is concerned.

The mother-liquor of A showed in

2.277 grms.

4205 grm. of ignited residue,

which gave $\cdot 2510$ of BaSO_4 ,
showing $\cdot 0857$ or $20\cdot 3$ per cent. of SO_3 in residue.

The mother-liquor of B showed in

$2\cdot 434$ grms.

$\cdot 4320$ grm. of residue,

which gave

$\cdot 2390$ „ of BaSO_4 ,

showing $\cdot 082$ or $18\cdot 8$ per cent. of SO_3 in residue.

§ 120. *Sulphate of Potassium and Chloride of Sodium as a Cryohydrate.*—The behaviour of the mixture of these salts also points to the non-existence of either.

$8\cdot 6$ grms. of K_2SO_4 ,

$5\cdot 85$ „ NaCl

($=\text{K}_2\text{SO}_4 + 2\text{NaCl}$)

were dissolved, mixed, concentrated to saturation, and cooled. At first granular crystals fell out. A cryohydrate began to form at -6° . The whole was dry at $-12^\circ\cdot 5$, whereas if NaCl had been present it would have required -22° . A mixture in the ratio $\text{K}_2\text{SO}_4 + \text{NaCl}$ behaved in a similar way, but was dry at -10°C .

§ 121. *Sulphate of Potassium and Chloride of Ammonium as a Cryohydrate.*—Similarly to § 120, a mixture in equivalent proportions of K_2SO_4 and NH_4Cl became quite dry at -12° . The salt NH_4Cl alone requires -15° .

§ 122. In the subjoined Table the results in regard to mixtures of salts as cryohydrates and cryogens are compared with the behaviour of their constituents in each capacity.

TABLE XV. Cryohydrates and Cryogens of equivalent mixtures of two salts.

Mixed salts.		Temperature of solidification of each constituent as cryohydrate.		Temperature of final solidification of mixture as cryohydrate.	Temperature as cryogen of mixture.
(a)	(b)	(a)	(b)	(a+b)	(a+b)
(1) KNO_3	$+\text{NaNO}_3 \dots$	$-2\cdot 6$	$-17\cdot 5$	-17	$-16\cdot 8$
(2) KCl	$+\text{NaCl} \dots\dots$	$-11\cdot 4$	$-22\cdot 0$	-21	$-21\cdot 8$
(3) KCl	$+\text{NH}_4\text{Cl} \dots$	$-11\cdot 4$	$-15\cdot 0$	-17	-18
(4) $\text{Ba}2\text{NO}_3$	$+\text{Sr}2\text{NO}_3 \dots$	$-0\cdot 8$	$-6\cdot 0$	$-4\cdot 3$	$-5\cdot 8$
(5) BaCl_2	$+\text{SrCl}_2 \dots\dots$	$-8\cdot 0$	$-17\cdot 0$	-18	$-16\cdot 7$
(6) $(\text{NH}_4)_2\text{SO}_4$	$+\text{Na}_2\text{SO}_4 \dots$	$-17\cdot 0$	$-0\cdot 7$	-7	-16
(7) 2KNO_3	$+\text{Na}_2\text{SO}_4 \dots$	$-2\cdot 6$	$-0\cdot 7$	-5	
(8) 2NaNO_3	$+\text{K}_2\text{SO}_4 \dots$	$-17\cdot 5$	$-11\cdot 2$	-5	
(9) K_2SO_4	$+2\text{NaCl} \dots$	$-1\cdot 2$	$-22\cdot 0$	$-12\cdot 5$	

This Table furnishes examples of the three typical kinds of relationship between the constituent salts and the mixture of them ; namely,

(a) In (1), (2), (4), (5) there appears to be no relationship between the salts. Thermally, the mixture may be considered as a mean. The salt of lower solidifying temperature prevents the solidification of the cryohydrate of its associate. The latter in its turn prevents the extreme cold being quite reached proper to the former when alone. In these cases the mixture, when used as a cryogen, gives a temperature a little above that of its more deeply depressive constituent. When jointly forming a cryohydrate, the complete solidification takes place at a slightly higher temperature than when the more strongly temperature-depressing constituent is alone. The variations from this may not be beyond the limit of observational error.

(b) In (3) and (7) double salts must be formed whose cryohydrates demand a lower temperature for solidification than does either of their constituents.

(c) Finally, in (8) and (9) the temperature of complete solidification is far above the solidifying temperature of that constituent whose solidifying temperature as a cryohydrate is lowest, where again (in 9) probably a double salt is formed, having, if our analogy hold, a greater water-worth than NaCl and a lesser one than K_2SO_4 .

Very remarkable is (6). Prone as the sulphates are to form double salts, we might indeed expect, as we find, a specific temperature for the cryohydrate. The difference of temperature of $a + b$ (6) when used as a cryogen ($-16^\circ C.$) and when solidifying as a cryohydrate (-7°), a difference of $9^\circ C.$, has at present its only counterpart in iodide of sodium (§§ 65, 68, 69), which as a cryogen has the temperature $-26^\circ.5$, as a cryohydrate -15° , a difference of $11^\circ.5$. Some chemists may not be unprepared for the suggestion that iodide of sodium may be in solution a double salt. To a few general considerations bearing on this and kindred points I may have to return subsequently. Meanwhile the general conclusion to be drawn from the above experiments is probably this. Nitrates of the alkalies and alkaline earths act together, but not in union (no double salts); chlorides may act in union with one another or with sulphates. Sulphates may act together or with chlorides or even with nitrates. The agreement of the composition of the several crops of cryohydrates of (3) (§ 113), which points to the relation of almost exactly $3KCl + 4NH_4Cl$, shows that in the society of solid water these chlorides have mutual equivalents, which, if not in simple relation to their so-called atomic weights, are yet equally definite and constant.

I have again to acknowledge my indebtedness to Mr. R. Cowper, who has been good enough to give me great assistance in the above experiments.

VIII. *On the Second Law of Thermodynamics in connexion with the Kinetic Theory of Gases.* By S. H. BURBURY*.

IN a memoir by Boltzmann† is contained an analytical proof of the second law of thermodynamics, founded on results obtained in the earlier part of the same memoir. The writer's treatment of this part of the subject is not characterized by his usual lucidity, and has given much trouble to some mathematicians. It is worth while to show that the second law can be deduced by a simple process from Boltzmann's results.

A great number, λ , of material particles are moving in a closed spherical vessel. Forces of mutual attraction or repulsion act between each pair of particles and between the particles and the walls of the vessel. To simplify the case, we will suppose that these are the only forces which act on the particles.

Boltzmann has shown that the mean kinetic energy is the same for every particle. Let T be this mean kinetic energy, and let $T = \frac{3}{2h}$. The mean kinetic energy of the system is then $\frac{3\lambda}{2h}$.

Let the positions of the particles at any instant be defined by reference to three rectangular axes passing through the centre of the vessel, the coordinates of the several particles being $x_1, y_1, z_1, \dots x_\lambda, y_\lambda, z_\lambda$.

Let v be the volume of the vessel.

The pressure exerted by the system of moving particles on any point on the surface of the sphere may vary from instant to instant according to the positions which the particles happen to have. But if the motion be stationary, this pressure must have on the average of any very long time a certain mean value P per unit area, which in the absence of impressed forces will be uniform over the whole surface.

Then P is the force per unit area which must be exerted on the surface of the sphere from without in order to maintain equilibrium.

Let χ be the ergal for any given positions of the particles of all the forces acting on the particles—that is, of all the forces which act in any manner on the system, except the external force P .

All those positions of the particles for which the coordinates of the first particle are between x_1 and $x_1 + dx_1$,

$$y_1 \text{ and } y_1 + dy_1,$$

$$z_1 \text{ and } z_1 + dz_1,$$

* Communicated by the Author.

† *Sitzungsberichte der Wiener Akad.* vol. lxiii.

of the second particle between x_2 and $x_2 + dx_2$,
 y_2 and $y_2 + dy_2$,
 $\cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot$,
 $\cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot$,
of the λ th particle between x_λ and $x_\lambda + dx_\lambda$,
 y_λ and $y_\lambda + dy_\lambda$,
 z_λ and $z_\lambda + dz_\lambda$,

may, when $dx_1, dy_1, \dots dz_\lambda$ are very small, be considered as having the same value of χ for ergal, notwithstanding any variations of position within those limits.

Let all such positions be deemed to constitute one configuration, which we may call configuration M. Then, as Boltzman has shown, the time during which on the average of any very long time the system will be within configuration M is proportional to

$$\bar{\epsilon}^{hx} dx_1 dy_1 \dots dz_\lambda$$

Its actual value is therefore

$$\frac{\bar{\epsilon}^{hx} dx_1 dy_1 \dots dz_\lambda}{\iiint \dots \bar{\epsilon}^{hx} dx_1 dy_1 \dots dz_\lambda},$$

the integration extending over all possible configurations.. The mean value of any function, f , of the position of the particles (which we denote as usual by \bar{f}) is

$$\bar{f} = \frac{\iiint \dots f \bar{\epsilon}^{hx} dx_1 dy_1 \dots dz_\lambda}{\iiint \dots \bar{\epsilon}^{hx} dx_1 dy_1 \dots dz_\lambda}$$

and therefore

$$\bar{\chi} = \frac{\iiint \dots \chi \bar{\epsilon}^{hx} dx_1 dy_1 \dots dz_\lambda}{\iiint \dots \bar{\epsilon}^{hx} dx_1 dy_1 \dots dz_\lambda}$$

II. Now let the system receive a small accession of heat, δQ , and let $\delta h, \delta v$ be the consequent increments of h and v . We may suppose the alteration of volume to be effected by the vessel undergoing a uniform homogeneous strain, the centre remaining fixed. In that case the distance, r , between any two points in it beomes after the alteration θr , where θ is a ratio the same for every pair of points, and manifestly $\theta = 1 + \frac{1}{3} \frac{\delta v}{v}$.

We further assume that the external forces are so adjusted as to maintain the system in equilibrium in its altered state, and that such adjustment takes place without altering χ for any given configuration, consequently that no change takes

place in χ except as the equivalent of work done by or against the forces on change of position of the particles. Then δQ consists (1) of alteration of kinetic energy—that is,

$$\delta\left(\frac{3\lambda}{2h}\right) \text{ or } -\frac{3\lambda}{2h^2}\delta h;$$

(2) of alteration $\delta\bar{\chi}$ of $\bar{\chi}$; (3) of external work done, $P\delta v$ —that is,

$$\delta Q = -\frac{3\lambda}{2h^2}\delta h + \delta\bar{\chi} + P\delta v.$$

We may here observe that

$$\delta Q - P\delta v = -\frac{3\lambda}{2h^2}\delta h + \delta\bar{\chi},$$

which is an exact differential of a function of two variables, h and v . This proposition is proved in a different way by Rankine (see 'The Steam-Engine,' eighth edition, pp. 304–313). Again,

$$\frac{\delta Q}{T} = -\frac{\lambda}{h}\delta h + \frac{2h}{3}\delta\bar{\chi} + \frac{2h}{3}P\delta v; \quad . . . \quad (A)$$

and it is required to prove that this expression is an exact differential.

III. Consider the expression

$$\log \iiint \dots \bar{\epsilon}^{hx} dx_1 dy_1 \dots dz_\lambda,$$

in which the integration extends over all configurations; and let

$$\log \iiint \dots \bar{\epsilon}^{hx} dx_1 dy_1 \dots dz_\lambda = u.$$

Then δu denotes the whole change which takes place in u consequent on h becoming $h + \delta h$, and v becoming $v + \delta v$; that is,

$$\delta u = \frac{du}{dh}\delta h + \frac{du}{dv}\delta v.$$

Now for any given configuration the ergal χ is not altered by alteration of h , though the comparative frequency of the occurrence of such configuration is altered; therefore

$$\frac{du}{dh}\delta h = -\delta h \frac{\iiint \dots \chi \bar{\epsilon}^{hx} dx_1 dy_1 \dots dz_\lambda}{\iiint \dots \bar{\epsilon}^{hx} dx_1 dy_1 \dots dz_\lambda} = -\bar{\chi}\delta h.$$

We have now to find $\frac{du}{dv}\delta v$. Both in the original and in the altered volume the integration is to be extended over all possible configurations. Now for every configuration M in the original volume there is in the altered volume a corresponding configuration, which we may call M' ; and M' is to be

formed from M by altering the distance between each pair of points in the ratio $\theta : 1$ —that is,

$$\left\{ 1 + \frac{1}{3} \frac{\delta v}{v} \right\} : 1.$$

If, then, $u, \chi, dx_1, dy_1, \dots dz_\lambda$ be denoted in the altered volume by accented letters, we shall have

$$\begin{aligned} u' &= \log \iiint \dots \bar{\epsilon}^{hx'} dx'_1 dy'_1 \dots dz'_\lambda \\ &= \log \{ \theta^{3\lambda} \bar{\epsilon}^{hx'} dx_1 dy_1 \dots dz_\lambda \}, \end{aligned}$$

since every linear element $dx_1, dy_1, \&c.$ is altered in the ratio $\theta : 1$.

In the last expression for u' , χ' is to be treated as a function of $x_1, y_1, \dots z_\lambda$. Then

$$\begin{aligned} \frac{\delta u}{\delta v} \delta v &= u' - u = \log \frac{\iiint \dots \bar{\epsilon}^{hx'} dx'_1 dy'_1 \dots dz'_\lambda}{\iiint \dots \bar{\epsilon}^{hx} dx_1 dy_1 \dots dz_\lambda} \\ &= \log \left\{ \theta^{3\lambda} \frac{\iiint \dots \bar{\epsilon}^{hx'} dx_1 dy_1 \dots dz_\lambda}{\iiint \dots \bar{\epsilon}^{hx} dx_1 dy_1 \dots dz_\lambda} \right\} \\ &= 3\lambda \log \theta - h \cdot \frac{\iiint \dots \delta \chi \bar{\epsilon}^{hx} dx_1 dy_1 \dots dz_\lambda}{\iiint \dots \bar{\epsilon}^{hx} dx_1 dy_1 \dots dz_\lambda} \\ &= \lambda \frac{\delta v}{v} - h \delta v \cdot \frac{\iiint \dots \frac{d\chi}{dv} \bar{\epsilon}^{hx} dx_1 dy_1 \dots dz_\lambda}{\iiint \dots \bar{\epsilon}^{hx} dx_1 dy_1 \dots dz_\lambda} \\ &= \lambda \frac{\delta v}{v} - h \frac{\overline{d\chi}}{dv} \delta v, \end{aligned}$$

in which expression $\frac{\overline{d\chi}}{dv}$ denotes the mean for all configurations of the alteration of χ consequent on v becoming $v + \delta v$.

Collecting our results, we obtain

$$\delta u = \frac{du}{dh} \delta h + \frac{du}{dv} \delta v = -\bar{\chi} \delta h + \lambda \frac{\delta v}{v} - h \frac{\overline{d\chi}}{dv} \delta v. \quad (B)$$

IV. The system being in stationary motion must satisfy Clausius's equation

$$\frac{3}{2} P v = \frac{3\lambda}{2h} + \frac{1}{2} \overline{\Sigma \Sigma R r},$$

in which P is the force acting from without on the surface of the vessel, and $\frac{1}{2} \overline{\Sigma \Sigma R r}$ is the mean virial of all the other forces acting on the system—that is, of all the forces of which the ergal is χ .

Also in $\Sigma\Sigma Rr$, R is the repulsive force, r the distance between any two particles, or between a particle and a point on the surface, and the summation includes every pair of points between which any force acts.

Then $\Sigma\Sigma Rr$ is a function of position of the particles, having a definite value for each configuration, and $\overline{\Sigma\Sigma Rr}$ denotes the mean value of $\Sigma\Sigma Rr$ for any long time; and we might therefore write

$$\overline{\Sigma\Sigma Rr} = \frac{\iiint \dots (\Sigma\Sigma Rr) \bar{\epsilon}^{hx} dx_1 dy_1 \dots dz_\lambda}{\iiint \dots \bar{\epsilon}^{hx} dx_1 dy_1 \dots dz_\lambda}.$$

Now let

$$P = P_1 + P_2,$$

and let

$$\frac{3}{2}P_1v = \frac{3\lambda}{2h}, \quad \dots \dots \dots (1)$$

$$\frac{3}{2}P_2v = \frac{1}{2}\overline{\Sigma\Sigma Rr}. \quad \dots \dots \dots (2)$$

It follows from (1) that

$$hP_1 = \frac{\lambda}{v},$$

and

$$hP_1\delta v = \lambda \frac{\delta v}{v}.$$

Also from (2),

$$P_2 = \frac{1}{3}\overline{\Sigma\Sigma R \frac{r}{v}},$$

$$P_2\delta v = \frac{1}{3}\overline{\Sigma\Sigma R \frac{\delta v}{v} r} \\ = \overline{\Sigma\Sigma R\delta r}.$$

Now for each configuration

$$\Sigma\Sigma R\delta r = -\delta\chi = -\frac{d\chi}{dv}\delta v.$$

Therefore, taking mean values,

$$\overline{\Sigma\Sigma R\delta r} = -\frac{\overline{d\chi}}{dv}\delta v;$$

therefore

$$P_2\delta v = \overline{\Sigma\Sigma R\delta r} = -\frac{\overline{d\chi}}{dv}\delta v.$$

Therefore

$$hP_2\delta v = -h\frac{\overline{d\chi}}{dv}\delta v.$$

And we have seen that

$$hP_1\delta v = \lambda \frac{\delta v}{v}.$$

Therefore

$$hP_1\delta v + hP_2\delta v = \lambda \frac{\delta v}{v} - h \frac{d\bar{\chi}}{dv} \delta v;$$

that is,

$$hP\delta v = \lambda \frac{\delta v}{v} - h \frac{d\bar{\chi}}{dv} \delta v.$$

Substituting in (B), we obtain

$$\delta u = -\bar{\chi}\delta h + hP\delta v,$$

or

$$hP\delta v = \bar{\chi}\delta h + \delta u;$$

and substituting in (A), we obtain

$$\begin{aligned} \frac{\delta Q}{T} &= -\lambda \frac{\delta h}{h} + \frac{2}{3} h \delta \bar{\chi} + \frac{2}{3} \bar{\chi} \delta h + \frac{2}{3} \delta u \\ &= \delta \left\{ -\lambda \log h + \frac{2}{3} h \bar{\chi} + \frac{2}{3} u \right\}, \end{aligned}$$

an exact differential.

The expression $-\lambda \log h + \frac{2}{3} h \bar{\chi} + \frac{2}{3} u$ is what Rankine denotes by ϕ in his equation

$$JdQ = Td\phi.$$

The value which we have obtained for ϕ is the same as that obtained by Boltzmann in the memoir above referred to. But his expression for δQ is

$$\begin{aligned} & -\frac{3\lambda}{2h^2} \delta h + \delta \left\{ \frac{\iiint \dots \chi \bar{\epsilon}^{hx} dx_1 dy_1 \dots dz_\lambda}{\iiint \dots \bar{\epsilon}^{hx} dx_1 dy_1 \dots dz_\lambda} \right\} \\ & - \frac{\iiint \dots \delta \chi \bar{\epsilon}^{hx} dx_1 dy_1 \dots dz_\lambda}{\iiint \dots \bar{\epsilon}^{hx} dx_1 dy_1 \dots dz_\lambda}, \end{aligned}$$

which is not easily to be reconciled with the expression we have used.

V. We have assumed hitherto that no alteration takes place in χ except as the equivalent of work done by or against the forces. It is, however, conceivable that a change should take place in χ for one and the same configuration, especially if, in the adjustment of external pressure to maintain equilibrium, new forces be brought into play. Boltzmann appears to contemplate this case. We proceed to show that, provided χ be for each

configuration a function of h and v , the function $\frac{dQ}{T}$ is still an exact differential.

Let $\delta_2\chi$ or $\frac{d_2\chi}{dh}\delta h + \frac{d_2\chi}{dv}\delta v$ be the new alteration in χ , which is not the equivalent of work done, and let $\delta_2\bar{\chi}$ be the mean value of $\delta_2\chi$.

Then, as the alteration is supposed to take place without expense to δQ_1 , $\delta_2\bar{\chi}$ must be subtracted from the whole change of the ergal in the expression for δQ . The equation (A) then becomes

$$\frac{\delta Q}{T} = -\lambda \frac{\delta h}{h} + \frac{2}{3} h \delta_2\bar{\chi} - \frac{2}{3} h \delta_2\chi + \frac{2}{3} h P \delta v.$$

Under the circumstances (B) becomes

$$\begin{aligned} \delta u &= -\bar{\chi} \delta h + \lambda \frac{\delta v}{v} - h \frac{d\bar{\chi}}{dv} \delta v - h \frac{d_2\bar{\chi}}{dh} \delta h - h \frac{d_2\bar{\chi}}{dv} \delta v \\ &= -\bar{\chi} \delta h + \lambda \frac{\delta v}{v} - h \frac{d\bar{\chi}}{dv} \delta v - h \delta_2\bar{\chi}; \end{aligned}$$

and by substitution we obtain, as before,

$$\frac{\delta Q}{T} = \delta \left\{ -\lambda \log h + \frac{2}{3} h \bar{\chi} + \frac{2}{3} u \right\} = \delta \phi.$$

IX. On the Production of Spectra by the Oxyhydrogen Flame. By TASKER H. MARVIN, Brooklyn, N. Y.*

SIX or seven months since, in using the lime-light for spectroscopic work, I had the good fortune to discover a method by which bright-line spectra, without an accompanying continuous spectrum, could be produced at will by means of the oxyhydrogen flame.

I had been engaged in examining the spectrum of calcium superimposed upon the continuous spectrum, which is easily seen by bringing the collimator of a compound spectroscope to bear *laterally* upon the incandescent point of the line. I found that I could bring out all the bands of calcium and the sodium line doubled sharply and brightly, notwithstanding the brightness of the continuous spectrum. When, however, the proportions of hydrogen and oxygen were so adjusted that there was no excess of the latter, the bright lines of calcium and sodium disappeared and the continuous spectrum alone remained. The chemical reaction then seemed evident; the

* Communicated by the Author.

excess of hydrogen at the high temperature of the flame seemed to reduce the oxide of calcium, and an atmosphere of vapour of metallic calcium partially enveloped the cylinder, and hence the double spectrum.

Following the suggestion, I soon found a simple way to eliminate entirely the continuous spectrum, leaving the other remarkably bright, sharp, and persistent. I accomplished this by taking the light from a point above or quite outside of the incandescent point of lime. After various experiments, the method adopted for producing these results most satisfactorily was to prepare artificial cylinders perforated centrally, the opening being $\frac{1}{4}$ inch in diameter; this was slipped on the jet, which was a double one, the two jets of gas playing upon opposite sides of the inner surface of the hollow cylinder. This threw a flame directly upwards, from which the spectrum was obtained.

The incandescent points being within the cylinder, no continuous spectrum was possible; and the results produced by this device were in the highest degree satisfactory.

It was evident, if lime could thus be reduced and the metal volatilized, that other oxides could be similarly treated; I therefore had hollow cylinders of strontia prepared, from which I obtained the strontia spectrum with *far greater* brightness and persistency *than is possible by ordinary means*. Indeed, so far as my experiments have gone, I see no reason why this method is not applicable to most of the metallic oxides. In experimenting, however, with an easily fusible oxide, like that of copper, I have found it necessary to combine it with some very refractory substance, like alumina, in order to prevent fusion.

In applying this method to the projection of spectra, the results were very satisfactory, and they warrant the expectation that the projection of most spectra may be effected in this way. My experiments in this direction have been limited for want of time.

An attempt to photograph the lines projected to a distance of 2 or 3 feet was sufficiently successful to satisfy me of the practicability of the operation.

I should say that having called the attention of my friend Professor A. K. Eaton to these facts, he has willingly lent his aid in the prosecution of these experiments, particularly in the line of projection.

Hoboken, New Jersey, Dec. 16, 1875.

X. *On a new fundamental Law of Electrodynamics.*

By Professor R. CLAUSIUS*.

IN order to explain electrodynamic phenomena, W. Weber, as is well known, advanced a law on the force exerted on each other by two moving particles of electricity. Let e and e' be the two particles, each of which may be either positive or negative, and let r be their distance from each other, which is to be regarded as a function of the time t ; according to Weber these particles exert a repulsion upon one another which is represented by the formula

$$\frac{ee'}{r^2} \left[1 - \frac{1}{c^2} \left(\frac{dr}{dt} \right)^2 + \frac{2}{c^2} r \frac{d^2r}{dt^2} \right],$$

in which c is a constant.

Against this formula objections have been raised by Helmholtz; and from reasons quite independent of Helmholtz's, the conviction has forced itself upon me that it does not correspond to the reality. On the other hand, my considerations have led me to another dynamic law, which I take leave to communicate provisionally, reserving for a subsequent occasion the complete exposition of the reasons which have induced me to advance it. Two remarks only, serving for elucidation, I must premise before introducing the formulæ.

If we start from the conception that the electrodynamic action upon each other of two moving particles of electricity takes place through an intervening substance, we need not assume that it is dependent only on the relative motion of the particles, but we can also ascribe an influence upon it to the absolute motions of the two individual particles. If, for instance, two electric particles are moving in parallel directions with equal velocity, and consequently are, relatively to one another, at rest, they may yet exert a reciprocal electrodynamic action; for to the medium existing between them they behave differently from particles actually at rest. Further, with this conception, we need not assume that the direction of the electrodynamic force coincides with the line connecting the two particles, but may also admit forces from other directions as possible.

Let, now, x, y, z and x', y', z' be the rectangular coordinates of the two electric particles e and e' (concentrated in points) at the time t ; and for the relative coordinates of the particle e to the particle e' let us introduce the following symbols—

$$\xi = x - x', \quad \eta = y - y', \quad \zeta = z - z'.$$

* Translated from a separate impression communicated by the Author, having been read before the Niederrheinische Gesellschaft für Natur- und Heilkunde, December 6th, 1875.

Call the distance between the two particles r . Further, let ds and ds' be two path-elements simultaneously passed through by the particles, ϵ the angle between them, and v and v' the velocities. If then the components (falling in the coordinate-directions) of the total electrostatic and electrodynamic force suffered by the particle e from the particle e' are represented by Xee' , Yee' , and Zee' , the following equations, written first in the most general form, hold good, in which k is a positive constant referred to the quantitative ratio between the electrodynamic and electrostatic portions of the force, and n another constant, of which we shall speak further on:—

$$X = \frac{\xi}{r^3} - k \left(\frac{\xi}{r^3} \cos \epsilon + n \frac{d^2 \xi}{ds ds'} \frac{r}{r} \right) vv' + k \frac{d}{dt} \left(\frac{1}{r} \frac{d\xi}{dt} \right);$$

$$Y = \frac{\eta}{r^3} - k \left(\frac{\eta}{r^3} \cos \epsilon + n \frac{d^2 \eta}{ds ds'} \frac{r}{r} \right) vv' + k \frac{d}{dt} \left(\frac{1}{r} \frac{d\eta}{dt} \right);$$

$$Z = \frac{\zeta}{r^3} - k \left(\frac{\zeta}{r^3} \cos \epsilon + n \frac{d^2 \zeta}{ds ds'} \frac{r}{r} \right) vv' + k \frac{d}{dt} \left(\frac{1}{r} \frac{d\zeta}{dt} \right).$$

From these equations all the forces and induction-actions exerted on one another by galvanic currents can be deduced.

The three components of the force exerted on a current-element ds by a current-element ds' are represented generally by the following expressions—

$$cii' ds ds' \left(-\frac{\xi}{r^3} \cos \epsilon + \frac{d\bar{r}}{ds'} \frac{d\xi}{ds} + \frac{d\bar{r}}{ds} \frac{d\xi}{ds'} - n \frac{d^2 \xi}{ds ds'} \frac{r}{r} \right),$$

$$cii' ds ds' \left(-\frac{\eta}{r^3} \cos \epsilon + \frac{d\bar{r}}{ds'} \frac{d\eta}{ds} + \frac{d\bar{r}}{ds} \frac{d\eta}{ds'} - n \frac{d^2 \eta}{ds ds'} \frac{r}{r} \right),$$

$$cii' ds ds' \left(-\frac{\zeta}{r^3} \cos \epsilon + \frac{d\bar{r}}{ds'} \frac{d\zeta}{ds} + \frac{d\bar{r}}{ds} \frac{d\zeta}{ds'} - n \frac{d^2 \zeta}{ds ds'} \frac{r}{r} \right),$$

in which i and i' signify current-intensities, and c is a positive constant dependent on the constant k and also on the unit selected for the measure of the current-intensity.

The question now is, what value is to be given to the constant n ? If the value 1 be chosen, the preceding expressions will represent the components of the same force that was de-

duced by Ampère, namely an attraction of the strength

$$civ' ds ds' \left(\frac{\cos \epsilon}{r^2} + r \frac{d^2 \frac{1}{r}}{ds ds'} \right).$$

This Ampèrian formula, however, cannot be proved by experiment for two single elements of current, but the experimental proof always refers to cases in which at least one of the currents is closed. Hence other forces may be regarded as admissible between two current-elements, if, for the case in which one current is closed, they only give the same result that was given by Ampère's calculation. Such forces are obtained when various values are given to the constant n in the above expressions; for the term affected by the factor n , occurring in each of the three expressions, which is a differential coefficient of the second order according to s and s' , becomes in the integration over a closed current zero, and consequently cannot have any influence on the force which a closed current exerts on a current-element.

Hence, if we admit as certain only that which has been confirmed by experiment, we can provisionally consider n a constant yet to be determined. Theoretically, however, that value is the most probable which makes the fundamental equations simplest, viz. the value 0, by which those equations are transformed into

$$X = \frac{\xi}{r^3} (1 - k vv' \cos \epsilon) + k \frac{d}{dt} \left(\frac{1}{r} \frac{d\xi}{dt} \right),$$

$$Y = \frac{\eta}{r^3} (1 - k vv' \cos \epsilon) + k \frac{d}{dt} \left(\frac{1}{r} \frac{d\eta}{dt} \right),$$

$$Z = \frac{\zeta}{r^3} (1 - k vv' \cos \epsilon) + k \frac{d}{dt} \left(\frac{1}{r} \frac{d\zeta}{dt} \right).$$

XI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

June 17, 1875.—Joseph Dalton Hooker, C.B., President, in the Chair.

THE following Papers were read:—

“On a new Form of Dynamo-Magneto-Electric Machine.”
By S. C. Tisley.

In the first machines constructed by Siemens and Wheatstone in

1867 (see Royal Society's Transactions) the power of augmenting the magnetism by successive currents, developed from the original residual magnetism contained in the iron, was fully demonstrated, and it was shown that the power of the machine could thereby be developed to a great extent; but the only means for obtaining external work was by the insertion in the circuit of a magnet or coil so that the secondary discharge could be utilized. Sir Charles Wheatstone also showed that a great part of the current could be shunted through a platinum wire, care being taken that the resistance of the platinum wire was sufficient to compel a large part of the current to pass round the electromagnet.

In the same year the writer designed a machine which was made by Mr. Ladd, and described by him in a paper read before this Society (see Transactions), the principle of which was that two separate armatures being introduced, one was employed for magnetizing the machine, the other being used for external work. This machine gave a good electric light &c., and was shown in the Exhibition of Paris, 1867, when a silver medal was awarded for it.

To simplify this machine, the author of this paper afterwards placed the two armatures in the same groove between the poles of the electromagnet, bolting the two together at right angles to each other, so that they came under the influence of the magnetism alternately; by this method one pair of bearings was sufficient instead of two, and the machine altogether was much simplified.

The machine now about to be described is a still further modification, in which the greatest amount of simplicity and effective power are combined.

The apparatus consists essentially of an electromagnet with shoes, forming a groove in which a Siemens's armature is made to revolve: this is much the same as the original machines made by Siemens and Wheatstone; but the difference occurs in the break or commutator; here there are two springs or rubbers employed in taking the current off from the commutator. The commutator consists of three rings: one of these rings is complete for three quarters of the circle, the other quarter being cut away; another ring is cut away three quarters, leaving the one quarter; and in between these two rings is a third ring, insulated and connected with the insulated end of the wire wound round the armature; on this centre ring are projecting pieces, one a quarter of a circle and the other three quarters, so arranged as to complete the two outer circles. The rubber spring which comes into contact with the quarter of the middle circle is connected with the electromagnet of the machine, and the armature is so arranged that at the time of contact the best magnetizing current is developed. The other spring rubber is in connexion with the wire on the armature during the other three quarters of its revolution; and this is connected with any external piece of apparatus required to be worked.

By this arrangement, the alternate currents being utilized, they are all in the same direction; and by the length of contact the

whole of the current is obtained in the best condition for heating wires, decomposing water, giving an electric light, and other usual experiments.

At present a model machine has been constructed on this principle, the armature of which measures 5 inches long by 2 inches diameter, on which is wound about 50 feet of cotton-covered copper wire, no. 16, B. W. G. The magnet has about 300 feet of covered copper wire, no. 14, B. W. G.: the whole instrument, without the driving-gear, weighs 26 lbs.; with this apparatus 8 inches of platinum wire, .005, can be made red-hot, water is rapidly decomposed, &c.

The armature is constructed specially to prevent the accumulation of heat to which every class of dynamo-magneto-electric machine is liable. It is made in two halves, a groove of a zigzag form being cast in each half, so that when the two are screwed together a continuous channel is maintained through the bearings for a current of cold water to pass during the whole time the machine is at work.

The advantages suggested by these arrangements are their extreme simplicity, the few number of parts, only one armature and one wire being used.

This principle of the alternate current being utilized is also applicable to machines constructed on the multiple armature principle; and the economy thereby resulting would prove of great advantage, as the power of the machine could be varied by throwing into the electromagnets either every other current, or every fourth, sixth, or eighth current, according to the strength required in the machine, the whole of the other currents being utilized for electric light or otherwise.

“On the Determination of Verdet’s Constant in Absolute Units.”
By J. E. H. Gordon, B.A., Gonville and Caius College, Cambridge*.

In the year 1845 Faraday discovered that certain media possess the property of rotating the plane of polarization of light passing through them when a magnetic force acts on them. About the year 1853 M. Verdet found that with the same magnet and medium the rotation is directly proportional to the strength of the magnet—that is, that the ratio between the amount of rotation and the intensity of the magnetic field is constant.

The object of this investigation is to measure this constant in absolute units for a standard substance. Distilled water was used, and the magnetic force was produced by means of an electric current in a helix, as the magnetism of iron magnets is an undetermined function of the shape and nature of the iron core.

The strength of the helix was determined by comparing the magnetic force at a series of seven equidistant points along its

* The whole of this work has been done under Prof. Clerk Maxwell’s superintendence. He suggested the method and nearly all the details; and any merit which the investigation may have belongs to him. He is, however, in no way responsible for any errors there may be in the numerical results.

axis in terms of that at the centre of the great dynamometer of the British Association, whose power is known in absolute measure.

The intensities were compared by varying currents sent opposite ways through each, till the action on a small magnet at their common centre was *nil*.

The intensity at each of a series of points being known for a given current, the difference of magnetic potential at the two ends for that current was obtained by integrating with respect to the length between limits corresponding to the end of the helix.

For this Weddle's rule was used, viz.

$$\int_0^h u_x dx = \frac{3}{10} h \{u_0 + u_2 + u_4 + u_6 + 5(u_1 + u_5) + 6u_3\},$$

where 6^h is the length of the helix and u_x the magnetic intensity at any point.

The difference of magnetic potential at the ends for a certain current being known, the strength, N , of the helix (which is the ratio of this difference to the current, or the difference of magnetic potential which would be due to a unit current) is known, and is a *number*, because current and magnetic potential are of the same dimensions.

In the helix used, which was about 26.34 centims. in length and 13 centims. in diameter, we had

$$N = 10752.$$

The absolute value of the degrees of a tangent galvanometer was also determined by placing it under the dynamometer.

To determine the rotation of the plane of polarization, a Nicol's prism, set in a circle, was used, and the light was polarized by means of a prism invented by Professor Jellett, and described by him in vol. xxv. of the Transactions of the Royal Irish Academy.

It was constructed of Iceland spar, and its field of vision consisted of a circle divided by a line, the light of one half of which was polarized in a certain plane, and the light of the other half in a plane inclined at about 2° to that of the first. The intermediate position of the Nicol, when the whole field was equally dark, could be determined with some accuracy.

The water was contained in a tube with glass ends, of the same length as the helix, and placed with it. The polarized ray was sent through it, and a current, whose intensity, C , was measured by the tangent galvanometer included in the circuit, was sent through the helix first in one direction and then in the other, and the plane of polarization observed. Half the difference of the readings was the rotation produced by the current.

If we call θ this rotation expressed in circular measure, and define Verdet's constant as the rotation which a unit current in a unit coil could produce in unit of length of distilled water, we have

$$\omega = \frac{\theta}{NC}$$

The result of the series of experiments made was to obtain for ω the value

$$\omega = (10^{-7}) 4.49 \text{ centimetre-gramme-seconds.}$$

Its dimensions obviously are the reciprocal of those of current, viz.

$$[\omega] = [L^{-\frac{1}{2}} M^{-\frac{1}{2}} T].$$

If we put our result in a slightly different form we may say that,

If plane polarized light passes through distilled water, and the magnetic potential of the water at any two points in the path of the ray differs by unity, then the plane of polarization will be rotated between those points $4\frac{1}{2}$ ten-millionths of a unit of circular measure.

Cavendish Laboratory, Cambridge,
April 30, 1875.

“On Rolling-Friction.” By Professor Osborne Reynolds.

The motion of a roller or wheel on a surface is always attended with resistance. Coulomb made some experiments with wooden rollers on a wooden plane, from which he deduced two laws, viz. that the resistance is proportional to the weight of the roller, and inversely proportional to its diameter. These laws have since been found to apply to other substances, a different coefficient being used in each case. Beyond this, however, nothing appears hitherto to have been ascertained as regards the nature of this resistance to rolling. The source from which it springs does not appear to have been made the subject of investigation.

Some time ago it occurred to the author that it was probable that the deformation of the surface of the roller and of the plane, which must take place at the point of contact, would affect the distance which the roller would advance in turning through a certain angle*. The pressure of the roller on the plane causes a certain temporary indentation and lateral extension in the latter, so that in passing from one point to another the roller does in truth pass over a greater extent of surface than the distance between these points. A simple experiment was sufficient to verify the truth of this conclusion. An iron roller 18 inches in circumference was found to roll through something like $\frac{3}{4}$ inch less than a yard in two complete revolutions when rolling on a plate of india-rubber. The softness of the india-rubber suffered the roller to indent it considerably; and hence it might be expected that the effect would be much more apparent than when the roller was rolling on iron or any hard material. At the same time there is doubtless a certain amount of indentation in this latter case; and this will probably cause a similar alteration in the distance rolled through, although too small to allow its being measured.

This falling off from what may be called the geometrical distance, suggested an explanation of the resistance to rolling, namely, that the extension of the surface or surfaces at the point of contact

* The Engineer, 27th Nov., 1874.

causes the one surface to slide over the other ; and this sliding is accomplished against friction. In this way we should expect to find the resistance to rolling greatest under those circumstances in which the sliding is greatest, *i. e.* where the indentation is greatest ; and so far it is in accordance with Coulomb's laws. In the case of india-rubber, we find the slipping is very large ; and hence we should expect the resistance to rolling to be large also ; and accordingly we find it so, for it is more than ten times as great as when the roller is on an iron plane. This very great resistance which india-rubber causes to rolling appears not to have previously caught attention ; and yet it is the natural explanation of the invariable failure which has attended the numerous endeavours which have been made to use this material for the tires of wheels.

This idea, that the resistance to rolling is due to the friction between the surfaces sliding at the point of contact, naturally leads to the conclusion that it must depend on the coefficient of friction between these surfaces, and that we might expect to diminish the resistance by using oil or any other means of reducing the coefficient of friction. This was the author's first impression. Experiments, however, showed that the effect of oiling the surface, although it did generally reduce the resistance, was very small ; and sometimes it appeared to act in the reverse manner, and increase the resistance. This conclusion or surmise was therefore wrong ; and the cause of the error was not far to seek. It consisted in having overlooked the fact that friction not only opposes the sliding of the one surface over the other, but also prevents it to a considerable extent, and thus modifies the deformation which would otherwise take place ; so that any diminution in the coefficient of friction is attended with an increase in the extent of slipping, which tends to balance the advantage gained by the reduced coefficient.

The truth of this view derives independent support from a circumstance remotely connected with rolling-friction, of which it furnishes an explanation. When the roller rests on a horizontal surface and is very slightly disturbed, it does not move off, but oscillates backwards and forwards. This happens on all kinds of elastic surfaces ; on soft india-rubber the oscillations are both large and continue for some time. Now if the deformation in the surface of the roller were complete, there would be no tendency to bring the roller back ; but since, owing to friction, the india-rubber, under the advancing side of the roller, is prevented from extending while that under the other side is prevented from contracting, there will exist a state of constraint from which the surface is endeavouring to free itself by forcing the roller back.

Besides the relative softness of the materials, the curvature of the roller will affect the lateral extension both of the roller and the plane at the point of contact, so that if the roller and the plane were of the same material there would still be slipping. This would not be the case, however, between two wheels of the same diameter and material rolling in contact.

Such is a short sketch of the subject of the paper, a considerable

part of which is devoted to the examination and illustration of the exact manner in which the deformation at the point of contact occurs, and the influence of friction upon it. The latter part of the paper contains an account of numerous experiments, and their results, which were undertaken as part of this investigation.

The first series of experiments relate to the resistance which an iron roller experiences on surfaces of different hardness. Cast iron, glass, brass, boxwood, and india-rubber were tried. Extreme care was taken to make the roller and the surfaces true; and this was so far successful that on cast iron the roller would roll in either direction when the surface had an inclination of one in five thousand, or, roughly, a foot in a mile. Comparing the different surfaces, we see that the resistance increases with the softness, although apparently not in the simple proportion; on boxwood the resistance is nearly double as great as on the harder surfaces, and on india-rubber from six to ten times as great.

The second series of experiments were to ascertain the actual extent of slipping on india-rubber, both with a cast-iron roller and also with an india-rubber tire glued on to the roller, and rolled on hard surfaces and on plates of india-rubber of different thicknesses.

These experiments bear out the arguments expressed in the first part of the paper; in fact the arguments were based on the experiments. There is no intention to imply that the whole of the resistance to rolling is in all cases due to the causes already mentioned. Under ordinary circumstances the irregularities of the surfaces and the crushing of the material beneath the roller are the chief causes. And, besides these, two other causes are discussed in the paper as having been brought to light by the experiment, viz. the communication of heat between the compressed material and that which surrounds it, which prevents the material immediately expanding to the same volume as it previously occupied, and the viscosity of the material, which also renders it slow to expand. Both these causes are, however, rather connected with the effect of the speed of the roller on the resistance than with the residual resistance, which, so far as the surfaces are perfectly true and perfectly hard, appears to be due to the friction which accompanies the deformation, and is hence called *rolling-friction*.

No attempt has yet been made to investigate the laws of rolling-friction, although the author hopes to continue the investigation in this direction as soon as he has obtained the necessary apparatus.

At the end of the paper attention is called to certain phenomena connected with railway-wheels, which it is thought now, for the first time, receive an explanation. Thus the surprising superiority of steel rails over iron in point of durability is explained as being due as much to the fact that their hardness prevents the wearing-action, *i. e.* the slipping, as that it enables them better to withstand the wear. Also the slipping beneath the wheel explains the wear of the rails in places where brake are not applied; and the severe lateral extension beneath the wheel is thought to explain the scaling of wrought-iron rails.

“ Preliminary Notice of further Researches on the Physical Properties of Matter in the Liquid and Gaseous States under varied conditions of Pressure and Temperature.” By Dr. Andrews, F.R.S.

The investigation to which this note refers has occupied me, with little intermission, since my former communication in 1869 to the Society, “ On the Continuity of the Liquid and Gaseous States of Matter.” It was undertaken chiefly to ascertain the modifications which the three great laws discovered respectively by Boyle, Gay-Lussac, and Dalton undergo when matter in the gaseous state is placed under physical conditions differing greatly from any hitherto within the reach of observation. It embraces a large number of experiments of precision performed at different temperatures and at pressures ranging from twelve to nearly three hundred atmospheres. The apparatus employed is, in all its essential parts, similar to that described in the paper referred to; and so perfectly did it act that the readings of the cathetometer, at the highest pressures and temperatures employed, were made with the same ease and accuracy as if the object of the experiment had been merely to determine the tension of aqueous vapour in a barometer-tube. In using it the chief improvement I have made is in the method of ascertaining the original volumes of the gases before compression, which can now be known with much less labour and greater accuracy than by the method I formerly described. The lower ends of the glass tubes containing the gases dip into small mercurial reservoirs formed of thin glass tubes, which rest on ledges within the apparatus. This arrangement has prevented many failures in screwing up the apparatus, and has given more precision to the measurements. A great improvement has also been made in the method of preparing the leather-washers used in the packing for the fine screws, by means of which the pressure is obtained. It consists in saturating the leather with grease by heating it *in vacuo* under melted lard. In this way the air enclosed within the pores of the leather is removed without the use of water, and a packing is obtained so perfect that it appears, as far as my experience goes, never to fail, provided it is used in a vessel filled with water. It is remarkable, however, that the same packing, when an apparatus specially constructed for the purpose of forged iron was filled with mercury, always yielded, even at a pressure of 40 atmospheres, in the course of a few days.

It is with regret that I am still obliged to give the pressures in atmospheres as indicated by an air- or hydrogen-manometer, without attempting for the present to apply the corrections required to reduce them to true pressures. The only satisfactory method of obtaining these corrections would be to compare the indications of the manometer with those of a column of mercury of the requisite length; and this method, as is known, was employed by Arago and Dulong, and afterwards in his classical researches by Regnault, for pressures reaching nearly to 30 atmospheres. For this moderate pressure a column of mercury about 23 metres,

or 75 feet, in length had to be employed. For pressures corresponding to 500 atmospheres, at which I have no difficulty in working with my apparatus, a mercurial column of the enormous height of 380 metres, or 1250 feet, would be required. Although the mechanical difficulties in the construction of a long tube for this purpose are perhaps not insuperable, it could only be mounted in front of some rare mountain escarpment, where it would be practically impossible to conduct a long series of delicate experiments. About three years ago I had the honour of submitting to the Council of the Society a proposal for constructing an apparatus which would have enabled any pressure to be measured by the successive additions of the pressure of a column of mercury of a fixed length; and working drawings of the apparatus were prepared by Mr. J. Cumine, whose services I am glad to have again this opportunity of acknowledging. An unexpected difficulty, however, arose in consequence of the packing of the screws (as I have already stated) not holding when the leather was in contact with mercury instead of water, and the apparatus was not constructed. For two years the problem appeared, if not theoretically, to be practically impossible of solution; but I am glad now to be able to announce to the Society that another method, simpler in principle and free from the objections to which I have referred, has lately suggested itself to me, by means of which it will, I fully expect, be possible to determine the rate of compressibility of hydrogen or other gas by direct reference to the weight of a liquid column, or rather of a number of liquid columns, up to pressures of 500 or even 1000 atmospheres. For the present it must be understood that, in stating the following results, the pressures in atmospheres are deduced from the apparent compressibility, in some cases of air, in others of hydrogen gas, contained in capillary glass tubes.

In this notice I will only refer to the results of experiments upon carbonic acid gas when alone or when mixed with nitrogen. It is with carbonic acid, indeed, that I have hitherto chiefly worked, as it is singularly well adapted for experiment; and the properties it exhibits will doubtless, in their main features, be found to represent those of other gaseous bodies at corresponding temperatures below and above their critical points.

Liquefaction of Carbonic Acid Gas.—The following results have been obtained from a number of very careful experiments, and give, it is believed, the pressures, as measured by an air-manometer, at which carbonic acid liquefies for the temperatures stated:—

Temperatures in Centigrade degrees,		Pressure in atmospheres.
0	35·04
5·45	40·44
11·45	47·04
16·92	53·77
22·22	61·13
25·39	65·78
28·30	70·39

I have been gratified to find that the two results (for $13^{\circ}09$ and $21^{\circ}46$) recorded in my former paper are in close agreement with these later experiments. On the other hand, the pressures I have found are lower than those given by Regnault as the result of his elaborate investigation (*Mémoires de l'Académie des Sciences*, vol. xxvi. p. 618). The method employed by that distinguished physicist was not, however, fitted to give accurately the pressures at which carbonic acid gas liquefies. It gave, indeed, the pressures exercised by the liquid when contained in large quantity in a Thilorier's reservoir; but these pressures are always considerably in excess of the true pressures in consequence of the unavoidable presence of a small quantity of compressed air, although the greatest precautions may have been taken in filling the apparatus. Even $\frac{1}{500}$ part of air will exercise a serious disturbing influence when the reservoir contains a notable quantity of liquid.

Law of Boyle.—The large deviations in the case of carbonic acid at high pressures from this law appeared distinctly from several of the results given in my former paper. I have now finished a long series of experiments on its compressibility at the respective temperatures of $6^{\circ}7$, $63^{\circ}7$, and 100° Centigrade. The two latter temperatures were obtained by passing the vapours of pyroxylic spirit (methyl alcohol) and of water into the rectangular case with plate-glass sides in which the tube containing the carbonic acid is placed. The temperature of the vapour of the pyroxylic spirit was observed by an accurate thermometer, whose indications were corrected for the unequal expansion of the mercury; while that of the vapour of water was deduced from the pressure as given by the height of the barometer and a water-gauge attached to the apparatus. At the lower temperature ($6^{\circ}7$) the range of pressure which could be applied was limited by the occurrence of liquefaction; but at the higher temperatures, which were considerably above the critical point of carbonic acid, there was no limit of this kind, and the pressures were carried as far as 223 atmospheres. I have only given a few of the results; but they will be sufficient to show the general effects of the pressure. In the following Tables p designates the pressure in atmospheres as given by the air-manometer, t the temperature of the carbonic acid, ϵ the ratio of the volume of the carbonic acid under one atmosphere and at the temperature t to its volume under the pressure p and at the same temperature, and θ the volume to which one volume of carbonic acid gas measured at 0° and 760 millimetres is reduced at the pressure p and temperature t .

Carbonic Acid at 6°·7.

<i>p.</i> at.		<i>t</i> °.		<i>c</i> .		<i>θ</i> .
13·22	6·90	$\frac{1}{14·36}$	0·07143
20·10	6·79	$\frac{1}{23·01}$	0·04456
24·81	6·73	$\frac{1}{29·60}$	0·03462
31·06	6·62	$\frac{1}{39·57}$	0·02589
40·11	6·59	$\frac{1}{58·40}$	0·01754

Carbonic Acid at 63°·7.

at.				$\frac{1}{17·85}$		0·06931
16·96	63·97	
54·33	63·57	$\frac{1}{66·06}$	0·01871
106·88	63·75	$\frac{1}{185·9}$	0·00665
145·54	63·70	$\frac{1}{327·3}$	0·00378
222·92	63·82	$\frac{1}{446·9}$	0·00277

Carbonic Acid at 100°.

at.				$\frac{1}{17·33}$		0·07914
16·80	100·38	
53·81	100·33	$\frac{1}{60·22}$	0·02278
105·69	100·37	$\frac{1}{137·1}$	0·01001
145·44	99·46	$\frac{1}{218·9}$	0·00625
223·57	99·44	$\frac{1}{380·9}$	0·00359

These results fully confirm the conclusions which I formerly deduced from the behaviour of carbonic acid at 48°, viz. that while the curve representing its volume under different pressures approximates more nearly to that of a perfect gas as the temperature is higher, the contraction is nevertheless greater than it would be if the law of Boyle held good, at least for any temperature at which experiments have yet been made. From the foregoing experiments it appears that at 63°·7 carbonic acid gas, under a pressure of 223 atmospheres, is reduced to $\frac{1}{447}$ of its volume under one atmosphere, or to less than one half the volume it ought to occupy if it were a perfect gas and contracted in conformity with Boyle's law. Even at 100° the contraction under the same pressure amounts to $\frac{1}{381}$ part of the whole. From these observations we may infer by analogy that the critical points of the greater number of the gases not hitherto liquefied are probably far below the lowest temperatures hitherto attained, and that they are not likely to be

seen, either as liquids or solids, till much lower temperatures even than those produced by liquid nitrous oxide are reached.

Law of Gay-Lussac.—That the law of Gay-Lussac in the case of the so-called permanent gases, or in general terms of gases greatly above their critical points, holds good at least at ordinary pressures, within the limits of experimental error, is highly probable from the experiments of Regnault; but the results I have obtained with carbonic acid will show that this law, like that of Boyle, is true only in certain limiting conditions of gaseous matter, and that it wholly fails in others. It will be shown that not only does the coefficient of expansion change rapidly with the pressure, but that, *the pressure or volume remaining constant, the coefficient changes with the temperature.* The latter result was first obtained from a set of preliminary experiments, in which the expansion of carbonic acid under a pressure of 17 atmospheres was observed at 4° , 20° , and 54° ; and it has since been fully confirmed by a large number of experiments made at different pressures and well-defined temperatures. These experiments were conducted by the two methods commonly known as the method of constant pressure and the method of constant volume. The two methods, except in the limiting conditions, do not give the same values for the coefficient of expansion; but they agree in this respect, that at high pressures the value of that coefficient changes with the temperature. While I have confined this statement to the actual results of experiment, I have no doubt that future observations will discover, in the case, at least, of such gases as carbonic acid, a similar but smaller change in the value of the coefficient for heat at low pressures. The numerous experiments I have made on this subject will shortly be communicated in detail to the Society; and for the present I will only give the following results:—

Expansion of Heat of Carbonic Acid Gas under high pressures.

Pressure. at.	Vol. CO ₂ at 0° & 760 millims.=1.	Vol. CO ₂ at $6^{\circ}05$ & 22.26 at.=1.	Temperature.	
22.26	0.03934	1.0000	$6^{\circ}05$	} . (A)
22.26	0.05183	1.3175	63.79	
22.26	0.05909	1.5020	100.10	

Pressure. at.	Vol. CO ₂ at 0° & 760 millims.=1.	Vol. CO ₂ at $6^{\circ}62$ & 31.06 at.=1.	Temperature.	
31.06	0.02589	1.0000	$6^{\circ}62$	} . (B)
31.06	0.03600	1.3905	63.83	
31.06	0.04160	1.6068	100.64	

Pressure. at.	Vol. CO ₂ at 0° & 760 millims.=1.	Vol. CO ₂ at $6^{\circ}01$ & 40.06 at.=1.	Temperature.	
40.06	0.01744	1.0000	$6^{\circ}01$	} . (C)
40.06	0.02697	1.5464	63.64	
40.06	0.03161	1.8123	100.60	

Taking as unit 1 vol. of carbonic acid at $6^{\circ}05$ and 22.26 atmospheres, we obtain from series A the following values for the coefficient of heat for different ranges of temperature:—

$\alpha = 0.005499$ from $6^{\circ}05$ to $63^{\circ}79$.

$\alpha = 0.005081$ from $63^{\circ}79$ to $100^{\circ}1$.

From series B, with the corresponding unit volume at $6^{\circ}62$ and 31.06 atmospheres, we find:—

$\alpha = 0.006826$ from $6^{\circ}62$ to $63^{\circ}83$.

$\alpha = 0.005876$ from $63^{\circ}83$ to $100^{\circ}64$.

And in like manner from series C with the unit volume at $6^{\circ}01$ and 40.06 atmospheres:—

$\alpha = 0.009481$ from $6^{\circ}01$ to $63^{\circ}64$.

$\alpha = 0.007194$ from $63^{\circ}64$ to $100^{\circ}60$.

The coefficient of carbonic acid under one atmosphere referred to a unit volume at 6° is $\alpha = 0.003629$.

From these experiments it appears that the coefficient of expansion increases rapidly with the pressure. Between the temperatures of 6° and 64° it is once and a half as great under 22 atmospheres, and more than two and a half times as great under 40 atmospheres, as at the pressure of 1 atmosphere. Still more important is the change in the value of the coefficient at different parts of the thermometric scale, the pressure remaining the same. An inspection of the figures will also show that this change of value at different temperatures increases with the pressure.

Another interesting question, and one of great importance in reference to the laws of molecular action, is the relation between the elastic forces of a gas at different temperatures while the volume remains constant. The experiments which I have made in this part of the inquiry are only preliminary, and were performed not with pure carbonic acid, but with a mixture of about 11 volumes of carbonic acid and 1 volume of air. It will be convenient, for the sake of comparison, to calculate, as is usually done, the values of α from these experiments; but it must be remembered that α here represents no longer a coefficient of volume, but a coefficient of elastic force.

Elastic force of a mixture of 11 vol. CO_2 and 1 vol. air heated under a constant volume to different temperatures.

Vol. CO_2 .	Temperature.		Elastic Force.	
			at.	
366.1	$13^{\circ}70$	22.90
366.2	$40^{\circ}63$	25.74
366.2	$99^{\circ}73$	31.65
256.8	$13^{\circ}70$	31.18
256.8	$40^{\circ}66$	35.44
256.8	$99^{\circ}75$	44.29

(A)

(B)

From series A we deduce for a unit at $13^{\circ}70$ and 22.90 atmospheres:—

$\alpha = 0.004604$ from $13^{\circ}70$ to $40^{\circ}63$.

$\alpha = 0.004367$ from $40^{\circ}63$ to $99^{\circ}73$.

And from series B:—

$\alpha = 0.005067$ from $13^{\circ}70$ to $40^{\circ}66$.

$\alpha = 0.004804$ from $40^{\circ}66$ to $99^{\circ}75$.

The coefficient at $13^{\circ}70$ and 1 atmosphere is $\alpha = 0.003513$.

It is clear that the changes in the values of α , calculated from the elastic forces under a constant volume, are in the same direction as those already deduced from the expansion of the gas under a

constant pressure. The value of α increases with the pressure, and it is greater at lower than at higher temperatures. But a remarkable relation exists between the coefficients in the present case which does not exist between the coefficients obtained from the expansion of the gas. The values of α , deduced for the same range of temperature from the elastic forces at different pressures, are directly proportional to one another. We have, in short,

$$\frac{0.004367}{0.004604} = 0.9485, \quad \frac{0.04804}{0.05067} = 0.9481.$$

How far this relation will be found to exist under other conditions of temperature and pressure will appear when experiments now in progress are brought to a conclusion.

Law of Dalton.—This law, as originally enunciated by its author, is, that the particles of one gas possess no repulsive or attractive power with regard to the particles of another. "Oxygen gas," he states, "azotic gas, hydrogenous gas, carbonic acid gas, aqueous vapour, and probably several other elastic fluids may exist in company under any pressure and at any temperature without any regard to their specific gravities, and without any pressure upon one another." The experiments which I have made on mixtures of carbonic acid and nitrogen have occupied a larger portion of time than all I have yet referred to. They have been carried to the great pressure of 283.9 atmospheres, as measured in glass tubes by a hydrogen manometer, at which pressure a mixture of 3 volumes carbonic acid and 4 volumes nitrogen was reduced at $7^{\circ}.6$ to $\frac{1}{3\frac{1}{8}}$ of its volume without liquefaction of the carbonic acid. As this note has already extended to an unusual length, I will not now attempt to give an analysis of these experiments, but shall briefly state their general results. The most important of these results is the lowering of the critical point by admixture with a non-condensable gas. Thus in the mixture mentioned above of carbonic acid and nitrogen, no liquid was formed at any pressure till the temperature was reduced below -20° C. Even the addition of only $\frac{1}{10}$ of its volume of air or nitrogen to carbonic acid gas will lower the critical point several degrees. Finally, these experiments leave no doubt that the law of Dalton entirely fails under high pressures, where one of the gases is at a temperature not greatly above its critical point. The anomalies observed in the tension of the vapour of water when alone and when mixed with air find their real explanation in the fact that the law of Dalton is only approximately true in the case of mixtures of air and aqueous vapour at the ordinary pressure and temperature of the atmosphere, and do not depend, as has been alleged, on any disturbing influence produced by a hygroscopic action of the sides of the containing vessel. The law of Dalton, in short, like the laws of Boyle and Gay-Lussac, only holds good in the case of gaseous bodies which are at feeble pressures and at temperatures greatly above their critical points. Under other conditions these laws are interfered with; and in certain conditions (such as some of those described in this note) the interfering causes become so powerful as practically to efface them.

XII. *Intelligence and Miscellaneous Articles.*

ON THE DISTRIBUTION OF MAGNETISM IN CIRCULAR OR ELLIPTIC PLATES OF STEEL. BY E. DUTER.

THE object of the following experiments was to ascertain the influence of the shape on the distribution of free magnetism in steel magnets. My examinations were effected upon plates of a circular or elliptic form, all cut out of the same steel and possessing the same temper, their thickness being one millimetre. The longest radii of the circles and longest axes of the ellipses are 15 centims. Magnetization was obtained by means of a very flat induction-coil formed by 400 metres of copper wire of 2 millims. diameter, through which passes the current of ten good Bunsen elements.

The method which I employed was that of M. Jamin; it is the only one which permits one to study any point one wishes of a magnet, by measuring in each point the separating-force of a very small contact of soft iron. To assure the complete success of the method in the case under consideration, two precautions are indispensable:—

In the first place, the magnet must have a perfectly polished and clean surface; the slightest trace of contamination tarnishing the magnet affects in a most irregular fashion the force which determines the separation. Further, every shock of the magnet must be avoided, even that of the little pulling-stud, which, if it falls suddenly on a point in the magnet, permanently alters the tension existing in that point.

The results I have obtained may be briefly stated thus:—

1. The total quantities of free magnetism spread over circles or ellipses are proportional to the surfaces.

2. This magnetism may be regarded as distributed over threads which affect the form of hyperbolas; the non-transverse axes of the hyperbolas are the axes of symmetry of the magnets, perpendicular to the neutral line. The transverse axes are directed along the neutral line; but their lengths vary from one thread to another.

On each of these threads, the law of distribution of the free magnetism is given by the formula

$$I = A(a^h - a^{-h}) :$$

I is the magnetic tension at the points under consideration; h is the rectified length of the arc of an hyperbola defined as I have said above, in which the point in question is situated; this length is reckoned from the middle zone. A and a vary from one thread to another, according to the two following laws:—

3. The tensions I_1 at the extremity of each hyperbolic thread are given by the formula

$$I_1 = \frac{L}{90^\circ} \text{arc tan } \frac{h_1}{k}, \text{ or } A(a^{h_1} - a^{-h_1}) = \frac{L}{90^\circ} \text{arc tan } \frac{h_1}{k} :$$

h_1 is the length of the hyperbola, reckoned from the middle zone to its extremity; L is the tension at the extremity of a thread of infinite length; k is another coefficient, which in the circles absolutely depends only on the nature of the temper of the steel; in the ellipses also it remains a constant number, provided the transverse

dimensions are not too small when compared with the axial dimensions.

4. The total quantities of magnetism spread over each hyperbolic thread are proportional to the squares of the tensions I_1 existing at the extremity of each of these threads—which leads to the formula

$$\frac{A}{La}(a^{h_1} + a^{-h_1}) - 2 = A^2 (a^{h_1} - a^{-h_1})^2.$$

Since finishing the investigation of which the above is a summary, I have undertaken some researches on magnets of very diverse forms; and I have found that the form of the threads in which the law $I = A(a^h - a^{-h})$ is maintained changes with the bounding curve of the magnets.

It is precisely in the investigation of these lines (which I call *homomagnetic lines*, and which, in the case of circles and ellipses, are hyperbolas) that the explanation is to be found of those still little-studied phenomena of distribution named *influence of the margins or the angles*.—*Comptes Rendus de l'Académie des Sciences*, vol. lxxxi. pp. 1099, 1100. —

NOTE ON THE TEMPERATURE OF THE UPPER STRATA OF THE
ATMOSPHERE. BY D. MENDELEEF.

A great number of atmospheric phenomena depend chiefly on the difference of temperature of the strata of the air; and so the quest of the theoretic and empirical laws of the variations of temperature with altitude has for a long time attracted the attention of several savants. Nevertheless even the theory of this question is still very imperfect.

The theory of the variation of temperature in the atmospheric strata rests on the supposition that the upper strata receive heat only from the lowest stratum, which is heated by the earth; the upper ones, therefore, would be cooled in dilating in consequence of diminution of pressure. Such a supposition contains implicitly this other, that air is diathermanous. For this case Poisson has demonstrated, between the final temperatures t_0 and t_1 when the pressure varies from H_0 to H_1^* , the relation

$$\frac{1 + \alpha t_0}{1 + \alpha t_1} = \left(\frac{H_0}{H_1}\right)^{\frac{k-1}{k}}, \quad \dots \dots \dots (1)$$

in which k is the ratio of two specific heats, 1.41. This formula, resting on the mechanical theory of heat, and agreeing with experiment, should, if the preceding hypothesis were accurate, be applicable to the determination of the temperatures t_1 of the strata of

* At first sight one might think that, in calculating the temperature of the upper strata, we ought to take into account not merely the heat expended for the dilatation due to the diminution of the pressure, but also that which is equivalent to the work of raising: the lowering of temperature would thus be doubled. This would be an error; for at the same time that a certain mass of air rises, another mass, equal to it, descends; otherwise equilibrium of the air would not take place: this is especially apparent when we consider, as we do now-a-days, the molecules of gases to possess a proper motion, the cause of diffusion &c.

the atmosphere when t_0 , H_0 , and H_1 are given. Now it conducts to temperatures too low. Thus, taking $H_0 = 750$ millims. and $t_0 = +15^\circ$, For $H_1 = \begin{matrix} 650 & 550 & 450 \\ \text{millims.} \end{matrix}$

We find $t_1 = +3^\circ.3 \quad -9^\circ.8 \quad -24^\circ.8$,

while Mr. Glaisher's observations (Reports of the British Association, 1862-1864) have given, for the same initial temperature and the same pressures:—

During the ascent of September 5, 1862 $+8^\circ.2 \quad +1^\circ.4 \quad -5^\circ.4$
Means of several balloon { with a clear sky . . . $+5^\circ.6 \quad -1^\circ.3 \quad -7^\circ.6$
observations { „ cloudy sky . . $+7^\circ.0 \quad +0^\circ.2 \quad -6^\circ.2$

Observations on mountains have led to similar results. M. Plantamour gives for Geneva the annual means $t_0 = 9^\circ.21$, $H_0 = 726.6$ millims.; for St. Bernard, where $H_1 = 563.9$, the annual temperature t_1 , calculated by aid of the relation (1), would be $-10^\circ.8$, while the observed temperature is $-1^\circ.76$. Formula (1), combined with the hypsometric formula, leads to the conclusion that, whatever H_0 and t_0 may be, a fall of 1 degree in the temperature will correspond

to an elevation z of 101.2 mètres*; that is to say, $\frac{\partial z}{\partial t} = -101.2$ mètres; and yet, in the Alps, the temperature only falls 1 degree for an elevation of 150 mètres in summer, and of 300 mètres in winter. Humboldt, Boussingault, and others have found that, in the tropics, an elevation of 180-250 mètres corresponds to a fall of 1° temperature.

Nevertheless it is beyond question that $\frac{\partial z}{\partial t}$ is a function of the initial temperature and pressure, but not a constant as indicated by formula (1).

The results to which this formula leads being very different from those given by observation, recourse is had in practice, and especially for the calculation of atmospheric refractions, to various interpolation formulæ with one or more constants, calculated from observations made in the higher strata (Laplace, Ivory, Kämtz, Bauerfeind, Kowalsky, &c.). On the other hand, the cause of the difference between the temperatures calculated by the relation (1) and the observed temperatures has been supposed to be found in the more perfect transparency of the upper strata, in the absorption of a portion of the heat of the sun by the atmosphere, &c. These hypotheses are gratuitous; their results cannot be submitted to calculation; and, besides, they are not required.

On comparing the data of the observations with those of formula (1), it is seen that there exists a source of heat in the upper strata of the atmosphere; for the observed are constantly higher than the calculated temperatures. This source is doubtless to be found in the aqueous vapour of the atmosphere. Two arguments in favour

* Formula (1) gives $\partial H = \frac{2}{1+\alpha t} \frac{H}{0.291} \partial t$; the differential equation which conducts to the hypsometric formula is $\partial H = -\frac{H}{7993} \frac{\partial z}{1+\alpha t}$ for lat. 45° , whence $\frac{\partial z}{\partial t} = -\frac{7993\alpha}{0.291} = -101.2$, the quantity z being the height expressed in mètres.

of this assertion will be developed in a second Note.—*Comptes Rendus de l'Académie des Sciences*, vol. lxxxi. pp. 1094–1096.

ON THE ACTION OF FLAMES IN PRESENCE OF ELECTRIFIED BODIES. BY M. DOULIOT.

A flame in communication with the earth discharges an electrified conductor placed near it; and if the flame is insulated, the discharge still takes place. In the first case the electricity of the conductor flows into the earth; but there is occasion for investigating what becomes of it in the second. It is with this view that the following experiments were undertaken.

1. A lighted taper, insulated, is placed at an equal distance from two electroscopes with gold leaves of equal dimensions, the knobs of the electroscopes and the flame forming a triangle of about 20 centims. length of side. If we bring an electrified body between the flame and one of the electroscopes, or into the interior of the triangle formed by these three bodies, the electroscopes are affected in the usual manner, and the gold leaves fall again as the body brought near loses its electricity. But if we place the electrified body in the prolongation of the line from one of the electroscopes to the flame, we see this electroscope become charged with electricity of the same kind as that lost by the electrified body; the other electroscope, although nearer to it, is merely influenced. The former retains its charge; the latter reverts to the natural state when the electrified body has discharged itself or is taken away.

2. The insulated flame is placed between a badly conducting screen and the knob of an electroscope, at about 15 centims. distance from each. Let us bring behind the screen a body charged with positive electricity; the gold leaves of the electroscope diverge instantly. We remove together both the screen and the electrified body; the electroscope remains electrified, and its electricity is positive. Nevertheless the electrified body has not been discharged; we can assure ourselves that it is still electrified positively; but we find also that the screen is charged with negative electricity on the face which was towards the flame and the electroscope.

If, instead of removing together the electrified body and the screen, we slowly take away the electrified body alone, we see the gold leaves, which received a charge of positive electricity at the instant when this body was brought near them, fall again, come into contact, then separate afresh; if we then withdraw the screen, we find that the electroscope remains electrified, that it contains negative electricity, and that the screen has reverted to its primitive state.

It would be difficult to explain all these facts by admitting that the flame and the bodies produced by it establish more or less perfect communication between the bodies which surround it. But it is to be remarked that these phenomena are, in all points, those which might be foreseen if for the flame we substituted an insulated conductor armed with points in all directions.—*Comptes Rendus de l'Académie des Sciences*, vol. lxxxi. p. 1208.

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XIII. *Experimental Proof that the Resistance to Galvanic Conduction is dependent on the Motion of the Conductor.* By E. EDLUND*.

I.

IN my memoir entitled "Théorie des Phénomènes Electriques" † I have endeavoured to prove, in the theoretical way, that the resistance to galvanic conduction must be proportional to the intensity of the current. This conclusion from theory contradicts the view, hitherto generally received, that the resistance to conduction is independent of the current-intensity. Nevertheless, on a closer investigation of the relations, we discover that the experiments which have been instituted do not by any means justify such an assumption; for I have shown, in the memoir just mentioned, that the various methods hitherto employed for measuring the resistance leave it in reality quite undecided whether the resistance is or is not dependent on the intensity of the current ‡. I have therefore instituted the following experiments for the purpose of solving the question experimentally.

If i signifies the current-intensity, a the cross section of the conductor, δ the mass of free æther in the unit of volume of the conductor (which mass, when a galvanic current is in question, may be regarded as invariable), and h the velocity of the

* Translated from a separate impression, communicated by the Author, from Poggendorff's *Annalen*, vol. clvi. pp. 251-278.

† *K. Svenska Vetenskaps Akademiens Handlingar*. Stockholm: Norstedt et Söner. Leipzig: F. A. Brockhaus.

‡ *Op. cit.* p. 25.

æther, we have by the theory $i = \delta ah$. Thus the current-intensity is measured by the mass of æther which flows through the cross section of the conductor in the unit of time: this I have called the mechanical measure of the intensity*. If r_0 denotes the resistance of the unit of length of the conductor when a current of unit intensity flows through it, then is r_0 the resistance in the usual signification of the term; and this resistance, in order to distinguish it from those which take place with other current-intensities, I have named the principal resistance to conduction†. Consequently, if r denotes the resistance with the current-intensity i , we have, according to the theory, $r = r_0 i$, or, eliminating i , $r = r_0 \delta ah$.

Theoretically, therefore, the resistance r is proportional to the *relative* velocity between the æther molecules and the molecules of the conductor. If, then, the conductor receive a velocity h_1 in the same direction in which the æther flows, the resistance r will be diminished; for it will become

$$r = r_0 \delta a (h - h_1).$$

If, on the contrary, we impart to the conductor the same velocity h_1 , but in the opposite direction, the resistance is augmented and we get

$$r = r_0 \delta a (h + h_1).$$

After the current-intensity has become constant, the electromotive force of the series must, according to the theory, be equal to the sum of all the resistances. If, then, the electromotive force and the principal resistance of the battery be denoted respectively by E and R_0 , and if r_0 signifies the principal resistance in the conductor which connects the poles of the battery, and i the intensity of the current, then is $E = R_0 i + r_0 i$, whence

$$i = \frac{E}{R_0 + r_0}.$$

If now to the conductor whose principal resistance is r_0 a velocity $\pm h$ is imparted, then, in accordance with what was said above, if i_1 signifies the current-intensity, it makes

$$E = R_0 i_1 + r_0 (i_1 \mp \delta ah),$$

whence

$$i_1 = \frac{E \pm r_0 \delta ah_1}{R_0 + r_0}.$$

Consequently the difference of the two currents becomes

$$i - i_1 =: \frac{\mp r_0 \delta ah_1}{R_0 + r_0} \quad . \quad . \quad . \quad . \quad (1)$$

* *Théorie des Phénomènes Electriques*, p. 10.

† *Ibid.* p. 24.

Therefore the change produced in the current-intensity by putting the conductor in motion is proportional to the velocity h_1 and the cross section a of the conductor. When the resistance R_0 is so inconsiderable that it may be neglected in comparison with r_0 , the alteration of the current-intensity is independent of the resistance.

At the first glance it might be supposed that the velocity h_1 which with accessible means can be imparted to the conductor would be so slight, in comparison with the velocity h of the æther, that the alteration in question (of the current-intensity) could not be observed even with the most delicate galvanometer. The explanation, however, given in the above-mentioned memoir (p. 56), of Quincke's diaphragm-currents, intimates that the observations must be practicable; and on this account I resolved to test the theoretical result by experiment.

II.

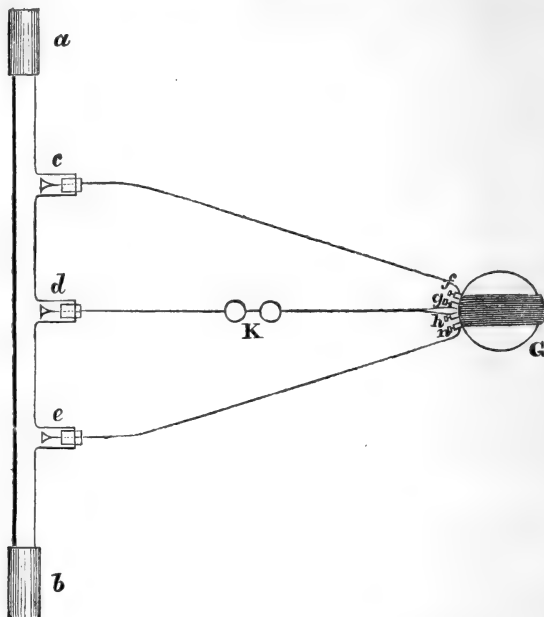
Special mechanical arrangements would be required if we would give to *solid* conductors the velocity necessary for these experiments. I therefore employed liquids, which can with facility be set in motion of sufficiently great velocity. But here two inconveniences appear, the injurious effects of which must be carefully avoided: one of them is the polarization of the plates by which the current is conducted into and out of the liquid; the other consists in the difficulty of making the two plates galvanically so nearly equal to each other that a feeble current shall not be produced by the inequality as soon as they are placed in the liquid. Yet neither the polarization of the plates nor their inequality in a galvanic point of view would injuriously affect the experiments, if these two conditions did not vary as soon as the liquid is put in motion. But it appears that such variation really takes place; and therefore the experiments cannot be made by simply sending a galvanic current through the liquid and measuring the intensity, first while the liquid is at rest, and again after it has been set in motion; for, as both the polarization and the galvanic quality of the plates are altered by the motion of the liquid, the observed difference in the current-intensity may just as well be attributed to this alteration as to a change in the resistance of the liquid.

After putting to the test of practice some proposed methods of observation which need not be here discussed, and finding them not advantageous, I finally adopted the following, which gave good results. (See fig. 1.)

ab is a cylindrical glass tube of uniform diameter and furnished with three side-tubes c , d , and e ; the distances between

c and *d* and between *d* and *e* are as nearly as possible equal. In each of these side-tubes a gold wire is fixed by means of a

Fig. 1.



cork and sealing-wax, terminated by a gold disk. On each end of the glass tube a metal cap with a screw-thread is fastened, by which the glass tube can be screwed on to the copper tube through which the liquid to be investigated flows into the glass tube. *G* represents the magnetometer used in the experiments, the almost perfectly astatic needle-system of which is suspended by a fine silver wire. The position of equilibrium of the needle-system is almost entirely independent of the variations of the earth's magnetic declination-plane, because it is all but exclusively determined by the torsion of the wire. The oscillation-period of the system amounts to about 28 seconds. By a thick copper sheath, which envelops one of the needles, so powerful a damping is produced that the system takes its position of equilibrium after a few oscillations. The reading-off is effected in the usual manner, by aid of telescope and scale. Although the magnetometer might possibly have been made more delicate by another arrangement, it possesses the great advantage of an almost invariable position of equilibrium—a circumstance of great value. In the experiments two of

the positions of the wire of the magnetometer were made use of, in which the resistance and the number of the turns were equal. In the one position the ends of the wire are connected with the binding-screws *f* and *g*, and in the other with *h* and *n*. At *K* an electromotor is inserted, one pole of which is connected with *g* and *h*, and the other with the gold wire in the middle side-tube *d*. The gold wires of the outer side-tubes *c* and *e* are united by means of a conducting-wire with *f* and *n*.

If the glass tube is filled with liquid, and this is at rest, the current from the electromotor divides itself between the two conductions, and these two portions of the current go through the turns of the magnetometer in opposite directions. The resistances of the two conductions are as equal as possible; and the polarization in the one must be just in the same proportion equal to that in the other, because the polarization of the gold disk in the middle side-tube exerts the same action upon both portions of the current, and the polarization of the two outer polar disks must be of the same nature and amount. Both portions must therefore be nearly the same in quantity, and the sum of their actions on the magnetometer become nearly equal to zero. The experiments also showed that the position of equilibrium of the needle-system differed by only a few scale-divisions from that occupied by the system when no current was passing through the windings of the magnetometer.

If, for instance, the liquid is moving from *a* to *b*, one portion of the galvanic current goes in the same direction as the flowing liquid, but the other portion goes in the opposite direction against the liquid stream. If the positive pole of the electromotor is connected with the gold disk in the middle side-tube, the stream of liquid and the portion *d e* of the galvanic current go in the same direction, but the portion *c d* in the reverse direction to that of the liquid; but if the negative pole of the series is connected with the same gold disk, the conditions are reversed. Now, if the resistance is dependent on the motion of the liquid (as the theory requires), the two portions of the current become unequal, and consequently the needle of the magnetometer must make a deflection, the direction of which can easily be determined beforehand. But there is another reason that the needle of the magnetometer may alter its position of equilibrium: as soon as the liquid is put in motion the polarization of the three gold disks is changed. Yet the change in the polarization of the middle plate has no influence upon the relative intensity of the two parts of the current, because this plate is common to both, and a change in the polarization must act with equal effect on both currents; it therefore cannot occasion a deflection of the needle. The outer plates do not behave in

the same manner. Although we try as much as possible to give them the same magnitude, form, and position, yet a slight difference in this respect may be present; and in consequence of it the variation in polarization effected by the flow of the liquid may be a little greater in one plate than in the other. Under all circumstances a difference between the two plates is present, consisting in the fact that during the flow of the liquid the upper disk is exposed to a greater pressure than the lower one; it is therefore possible that this may cause the needle to make a slight deflection. The deflection which results from variation of polarization, however, we can make as small as we please, by selecting for the experiments liquids the resistance of which is sufficiently great. Moreover it results from the experiments which have been made, that the *direction* of the deflection is determined exclusively by this—that the resistance of the liquid becomes less if it flows in the same direction as the galvanic current, but greater if the directions are opposite.

The form of the experiments was as follows:—When the positive pole of the electromotor was connected with the gold disk in the middle side-tube, as soon as the liquid was set moving from *a* to *b* I obtained a deflection which indicated that the part of the current passing through the liquid column *de* (therefore the portion that had the same direction as the flow of the liquid) possessed a greater intensity than the other. Now the cause of this might either be that the resistance in the column *de* became less, and in column *cd* greater, than when the liquid was at rest, or possibly that by the motion of the liquid the polarization of the polar disk *e* was rendered feebler than that of the disk *c*. If the change produced in the polarization of the one disk by the motion of the liquid actually differed from that produced in the polarization of the other, this must have depended on the disks in the side-tubes *c* and *e* differing somewhat in size, form, and position, or else it was because the one was exposed to a greater pressure than the other. Therefore it cannot be with safety concluded from a single experiment that the resistance to conduction is dependent on the motion of the liquid. Thereupon the glass tube was inverted, so that the liquid flowed from *b* to *a*, while all the other conditions remained unchanged. The direction of the deflection obtained was the opposite of the previous one, from which it follows that the part of the current which passed through the column of liquid from *d* to *c* (and consequently had the same direction as the liquid stream) was more intense than the other. From these two experiments it follows that the difference in form, size, and position that may have existed

between the two outer disks, and the alteration of polarization thereon depending, had no effect on the direction of the deflection. If, as in the first experiment, the liquid moved from a to b , and the negative instead of the positive pole of the electromotor was connected with the disk in the middle side-tube, the deflection obtained indicated that the part of the current which passed through the liquid column cd (and therefore had the same direction as the flow of the liquid) possessed a greater intensity than the other. In the first two experiments, therefore, the more powerful part of the galvanic current went between the middle polar disk and that one of the two outer disks which was exposed to the *less* pressure from the flowing liquid; but in the last experiment the more powerful current passed between the middle disk and that one of the two outer disks which received the *greater* pressure. The difference in the pressures to which the outer disks were exposed during the flow of the liquid cannot, then, have exerted any influence on the direction of the deflection. On the other hand, it appeared in all the experiments that that part of the current which had the same direction as the flow of the liquid was always the intenser; and for this result no other reason can be given than that the galvanic resistance becomes less when the current moves in the same direction as the liquid, and greater when the directions are opposite.

In order that the deflections may turn out regular, the liquid employed must exert a great resistance. The importance, in this respect, of the resistance will be seen from the following calculation.

We will denote by r the sum of the principal resistances in the liquid column cd , in the conducting-wire cf , and in one position of the wire of the magnetometer, and, in a corresponding manner, by $r + \rho$ the sum of the resistances in the liquid column de , in the conducting-wire en , and in the other position of the magnetometer-wire. Because these sums have been made as nearly as possible equal to one another, ρ must be a very small quantity. If we now assume that the negative pole of the electromotor is connected with the gold disk in the middle side-tube, and that the liquid employed is water, this disk covers itself with hydrogen, the electromotive force of whose polarization may be called w . The two outer disks, on the contrary, become coated with oxygen; and the electromotive force of its polarization in the side-tube c we will denote by s , and in the side-tube e by $s + \sigma$. As the polarization is all but perfectly equal in both these disks, the value of σ must be inconsiderable. Let i denote that part of the current which flows through the liquid column cd , and i_1 that part which

flows through $d e$. The electromotive force of the electromotor we will name E ; the principal resistance of it, R ; and the intensity of the undivided current, J . We have then, by Kirchhoff's laws for the division of the current between several conductors,

$$E - (w + s) = RJ + ir,$$

$$E - (w + s + \sigma) = RJ + i_1(r + \rho),$$

and

$$J = i + i_1.$$

From this we get

$$i_1 = \frac{Er - R\sigma - r(w + s + \sigma)}{2Rr + r^2 + R\rho + r\rho} \quad . \quad . \quad . \quad (2)$$

and

$$i = \frac{E(r + \rho) + R\sigma - (r + \rho)(w + s)}{2Rr + r^2 + R\rho + r\rho}, \quad . \quad . \quad (3)$$

and consequently

$$i - i_1 = \frac{E\rho + 2R\sigma + r\sigma - \rho w - \rho s}{2Rr + r^2 + R\rho + r\rho}. \quad . \quad . \quad (4)$$

The difference $i - i_1$ determines the position of equilibrium of the magnetometer-needle when the liquid is at rest. But when the liquid is set in motion, s is changed into $s + \beta$, $s + \sigma$ into $s + \sigma + \beta_1$, and w into $w + \pi$. Denoting the parts of the current which flow through the liquid columns cd and de respectively by i_{II} and i_{III} , we obtain in the same manner

$$i_{III} = \frac{Er + R\beta - R\sigma - R\beta_1 - r(w + \pi + s + \sigma + \beta_1)}{2Rr + r^2 + R\rho + r\rho} \quad . \quad (5)$$

and

$$i_{II} = \frac{E(r + \rho) + R\sigma + R\beta_1 - R\beta + (r + \rho)(w + \pi + s + \beta)}{2Rr + r^2 + R\rho + r\rho}, \quad (6)$$

whence $i_{II} - i_{III} =$

$$\frac{E\rho + 2R\beta_1 - 2R\beta + 2R\sigma + r\sigma + r\beta_1 - r\beta - \rho(w + \pi + s + \beta)}{2Rr + r^2 + R\rho + r\rho}. \quad (7)$$

Subtracting the first difference (4) from the last (7), we obtain the expression for the deflection which arises solely from the fact that the polarization is altered by the motion of the liquid. In this way we get

$$(i_{II} - i_{III}) - (i - i_1) = \frac{2R(\beta_1 - \beta) + r(\beta_1 - \beta) - \rho\pi + \rho\beta}{2Rr + r^2 + R\rho + r\rho}. \quad . \quad (8)$$

The value of ρ , however, is so inconsiderable that $\rho\pi$, $\rho\beta$, $R\rho$, and $r\rho$ can be neglected. The deflection u , which arises only from the alteration of the polarization by the motion of

the liquid, becomes therefore

$$u = \frac{\beta_1 - \beta}{r} (9)$$

This deflection is consequently inversely proportional to the resistance of the liquid ; while the deflection occasioned by the change in the resistance is, according to equation (1), nearly independent of the same resistance. If, then, liquids of sufficiently great resistance be selected for the experiments, we can make the variations in the polarization harmless without thereby lessening the deflections produced by the alteration of the resistance. Moreover it follows from equation (1) that the deflections in question are proportional to the cross section of the glass tube and to the velocity of the liquid, but independent of the intensity of the galvanic current employed.

Of the great number of observations conducted in the way above described, it may be sufficient for the purpose before us to communicate only the following.

III.

For the experiments a cylindrical vessel of thick sheet copper was used, tinned on the inside, and capable of holding about 25 litres. The upper end was hemispherical and provided with three apertures. In one of them a cock was fixed, which was connected with an air-condensing engine by means of a leaden pipe. In order to measure the condensation of the air produced by the engine, a manometer could be screwed on one of the other apertures, which was also made use of for the purpose of filling a part of the vessel with liquid. In the third aperture a copper pipe with a cock was fixed air-tight. That part of this pipe which was inside descended perpendicularly nearly to the bottom of the vessel ; the part outside was bent the form of a semicircle, so that its extremity descended perpendicularly. To this end of the copper tube the above-described glass tube was screwed. The liquid flowing through the glass tube was received in a vessel of glass or wood standing on the floor of the room. Before each experiment, air was forced into the vessel, above the liquid, up to a pressure of two or three atmospheres above the pressure of the external air. No measures were adopted to keep the pressure constant and invariable ; and on this account the velocity of the liquid grew less during the outflow. This inconvenience, however, was of little consequence, because in this investigation the question was rather to obtain an experimental proof of the dependence of the resistance on the motion of the liquid than an accurate determination of the laws of that dependence.

Experiments with distilled water.

Series 1.—The glass tube had an internal diameter of 5·5 millims.; the distance between the side-tubes was 70, and the length of the entire tube was 270 millims. The gold disks were drawn so far back in the side-tubes that the distance from the inner surface of the glass tube amounted to 1 centim. Trial was first made whether a deflection could be obtained merely by the motion of the liquid, without inserting an electromotor at K. In this the gold disk in the middle side-tube was connected immediately with the binding-screws *g* and *h* by a simple conducting-wire. As was to be expected, in this experiment not a trace of a current was observed.

A series of two Daniell's elements was now inserted at K. The rest of the conducting-wires were arranged as shown in fig. 1. The position of equilibrium occupied by the needle when the glass tube was filled with water at rest is denoted by A in the following. As soon as the cock was opened and the water set in motion, the magnetometer-needle made a deflection. The scale-division at which the needle, after a complete deflection, began to return, is given by B; and D gives the difference between A and B.

The negative pole of the electromotor was connected with the middle polar disk; the water-flow was from *a* to *b*:

A.	B.	D.
617·0	619·0	2·0
617·0	618·8	1·8
617·0	619·1	2·2
Mean . .		2·0

The deflection shows that the part of the galvanic current which passed from *c* to *d* (that is, in the same direction as the water-flow) was more powerful than the other.

The glass tube was now inverted, so that the water flowed through it from *b* to *a*; every thing beside remained unchanged:

A.	B.	D.
618·0	616·0	2·0
618·1	615·9	2·2
618·0	615·9	2·1
Mean . .		2·1

The deflection (the direction of which was opposite to that of the previous one) indicated that the part of the current which went from *e* to *d*, and therefore had the same direction as the water-flow, was the more powerful.

The positive pole was connected with the middle disk; the water flowed continuously from *b* to *a*:

A.	B.	D.
617·0	615·8	1·2
617·5	615·0	2·5
617·2	614·9	2·3
Mean . .		2·0

The deflection had now the same direction as in the next preceding experiment. It thence follows that the part of the current which coincided in direction with the water-flow, going from *d* to *c*, exerted the greatest action.

The glass tube was then inverted, so that the water flowed again from *a* to *b* ; all beside unchanged:

A.	B.	D.
617·0	619·2	2·2
617·8	620·4	2·6
618·0	621·0	3·0
Mean . .		2·6

Consequently it was shown here also that the portion of the current having the same direction as the water-stream was stronger than the other.

Series 2.—This was similar to the preceding, but with the difference that the disks in the side-tubes were not drawn so far back, and so were placed near the inner surface of the glass tube. They were more exposed, therefore, than before to the mechanical action of the flow of water.

The positive pole of the electromotor connected with the middle disk :

A.	B.	D.
628·8	631·8	3·0
628·8	632·0	3·2
Mean . .		3·1

The direction of the deflection showed that the current in the same direction as the water-flow possessed a greater intensity than the other.

After this the glass tube was inverted, so that the water flowed through it in the opposite direction ; all beside unchanged :

A.	B.	D.
628·9	626·0	2·9
628·7	625·9	2·8
628·9	625·2	3·7
Mean . .		3·1

The negative pole connected with the middle disk ; all else unchanged :

A.	B.	D.
631.3	629.0	2.3
631.5	629.0	2.5
631.5	629.0	2.5
Mean . .		2.4

Glass tube inverted ; the rest of the arrangement unchanged :

A.	B.	D.
632.0	634.0	2.0
631.9	634.0	2.1
631.9	634.2	2.3
Mean . .		2.1

Consequently this series gives the same result as the first—namely, that the resistance to galvanic conduction is diminished if the conductor moves in the same direction as the galvanic current, but it is increased if the two go in opposite directions.

In all these experiments the velocity of the distilled water was between 9 and 10.5 mètres in a second.

Experiments with alcohol containing by volume 24 per cent. water.

With this liquid the experiments were conducted in the same manner as with the distilled water, and led to the same result ; it might therefore be superfluous to describe them here.

Experiments with water from the town-supply.

An experiment was made which showed that the resistance of distilled water is about fifty times as great as the water from the conduit. A glass tube in all respects like the one before described was screwed on to the water-pipe in a vertical position. The connexions between the gold disks in the side-tubes, the electromotor, and the magnetometer were arranged in the same way as in the preceding experiments. The only difference with the observations was, that, as the supply of liquid was unlimited, the constant deflections were observed instead of the scale-division at which the magnetometer-needle began to return. By special experiments, in which no electromotor was inserted between the middle disk and the binding-screws *g* and *h*, I convinced myself that no galvanic current arose from the water out-flow alone. Therefore no extraneous cause of a galvanic current was present to produce a deflection of the galvanometer-needle.

As the water from the conduit was, in comparison with distilled water, a good conductor, it was to be expected that the polarization of the disks would here exercise a greater influence

than in the foregoing experiments ; this results clearly from equation (9). On this account, the experiments had to be conducted with suitable precautions to prevent the polarization from affecting the direction of the deflections. Before each experiment the electric series had to remain a longer time closed in order that the disks might be completely polarized. During this time the water-cock was several times turned on and off. It was only after this had been done that I could reckon upon obtaining constant and regular deflections. It must, besides, be remarked that the galvanic current employed should not be too powerful, because the harmful effect arising from the galvanic difference between the disks increases with the intensity of the current. In the following experiments two Daniell's elements were employed. Out of the observations thus conducted, it will be sufficient to give the two following series. A, B, and D have the same signification as before.

Series 3.—Here another glass tube was employed ; but the distance of the disks from each other and the diameter of the tube were about the same as in the first tube. The velocity of the water, when the cock was turned full on, amounted to about 10 mètres per second. In reckoning the magnitude of the deflections, the mean was taken of two successive positions of equilibrium with still water.

The negative pole of the electromotor was connected with the middle disk :

A.	B.	D.
608.0	611.0	2.6
608.8	611.2	2.3
609.0	610.2	1.2
609.0	610.5	1.5
609.0	611.0	2.0
609.9	Mean . .	1.92

The positive pole connected with the middle disk, the rest unchanged :

A.	B.	D.
626.5	628.8	2.3
626.5	627.5	1.7
625.0	628.5	3.0
626.0	627.5	1.7
625.5	627.2	1.2
626.4	Mean . .	1.98

The direction of the deflections indicated in both cases that the resistance is diminished when the galvanic current and the water-flow coincide in direction.

The glass tube was then inverted, so that the water entered at the other end of it, every thing else remaining unaltered :

A.	B.	D.
627·3	624·9	2·3
627·0	624·0	3·0
627·0	624·4	2·6
627·0	624·0	2·8
626·5	624·0	2·6
620·7	Mean . .	2·66

The negative pole was connected with the middle disk :

A.	B.	D.
606·4	605·0	1·6
606·8	603·0	3·9
607·0	605·4	1·9
607·5	605·4	1·9
607·0	604·8	2·2
607·0	Mean . .	2·30

In both the last two cases, the deflections were (as they should have been) opposite to the preceding ones.

Series 4.—The disks were drawn so far back in the side-tubes that the distance from the inner surface of the glass tube amounted to 8 millims. They were thereby less exposed to the mechanical action of the stream of water.

The negative pole was connected with the middle disk :

A.	B.	D.
626·0	623·0	2·0
624·0	622·6	2·4
626·0	622·8	2·7
625·0	622·5	2·0
624·0	622·5	1·5
624·0	Mean . .	2·12

The glass tube was inverted, so that the water entered at the other end ; all beside unchanged :

A.	B.	D.
625·5	629·0	4·0
624·5	629·0	4·7
624·0	629·0	4·7
624·5	628·5	4·0
624·5	629·0	4·7
624·0	Mean . .	4·42

The positive pole was connected with the middle disk, the rest unchanged :

A.	B.	D.
615·0	617·0	1·6
615·8	616·2	0·8
615·0	616·2	1·2
615·0	616·0	1·0
615·0	Mean . .	<u>1·15</u>

After this the glass tube was turned back again ; all beside unaltered. The following observations were made a day subsequent to the foregoing :—

A.	B.	D.
612·4	610·5	2·2
613·0	610·0	3·0
613·0	610·0	3·0
613·0	610·0	2·5
612·0	610·0	2·2
612·4	Mean . .	<u>2·58</u>

The directions of the deflections in this series were exactly the same as in the previous ones, indicating that the resistance to conduction is lessened when the conductor moves in the same direction as the galvanic current.

In the experiments here communicated the *direction* of the deflections is exclusively of importance, and too much weight must not be attributed to their absolute magnitude ; for the latter was altered partly by polarization. After the water is set in motion, the two parts into which the galvanic current divides are no longer equal : that which goes in the same direction as the liquid has become greater, while the other is diminished by an equal quantity ; and therefore currents of different intensities pass through the disks in the two outer side-tubes. As the polarization, when the currents are as feeble as they were in these experiments, increases with the current-intensity, the more intense part of the current must be somewhat more weakened by the polarization than the other ; and since the deflections are proportional to the difference of the two current-intensities, they must be smaller than when no polarization takes place. On the other hand, the polarization of the middle disk, as already remarked, has no effect on the relative quantity of the two parts of the current. Therefore no reliable measure of the change produced in the resistance of the liquid by its motion can be obtained from the experiments made ; they show only that such a change actually takes place, and that it cannot fall below the limit which can be calculated from the deflections obtained.

As already stated, we get no deflections, unless an electro-

motor is inserted between the middle disk and the binding-screws *g* and *h*; so that the deflections obtained do not derive their origin from any sort of diaphragm-currents or from any other extraneous cause. The following observation (to which I would direct the attention of those who may desire to control my experiments) does not contradict this:—If, as soon as a series of experiments is concluded, the electromotor be withdrawn and instead of it a simple conducting-wire connect the middle disk with the binding-screws *g* and *h*, and thereupon the liquid be set in motion, a deflection will be obtained, and this in the same direction as the one a short time before obtained while the electromotor was in the circuit; and if the glass tube be now inverted, we shall have, quite regularly, a deflection in the opposite direction. It is easy to perceive whence these deflections proceed. When the electromotor was inserted the disks in the side-tubes were polarized, and the two outer ones received a coating of the same sort of gas. Since the disappearance of the polarization is gradual, on this account a polarization-current went through each half of the water column immediately after the removal of the electromotor; but the current in one half was opposite to that in the other, in the same manner as when the electromotor was in the circuit; and when the liquid was put in motion, the resistance in one half became greater, and the other less, than before. Thereby the two currents acquired unequal intensity; and in consequence of this the magnetometer-needle could not but make a deflection.

IV.

The result of the above experiments I have endeavoured to control by a method of investigation in every respect different from that just described; it was the following:—

Kohlrausch and Nippoldt* have shown that alternating induction-currents, succeeding each other at sufficiently brief intervals, can be advantageously employed for making the resistance of liquids independent of polarization. When water is the electrolytic liquid, only a very thin covering of oxygen can form on the one polar disk, and of hydrogen on the other, during the short time that the current lasts; and in the next instant the directions of the current are reversed, so that the disk which just before was receiving hydrogen receives now oxygen, and *vice versâ*. If the currents are of equal intensity and short duration and follow close upon one another, it is easy to see that the polarization produced by them must be reduced to a minimum. Indeed the investigations of the above-mentioned philosophers showed also that the polarization was without per-

* Pogg. *Ann.* vol. cxxxviii. p. 280.

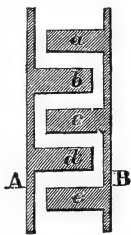
ceptible influence on the accuracy of the measurements of the resistance.

We will imagine that the polar disks in the outer side-tubes *c* and *e* (fig. 1) are connected with the windings of the magnetometer in such wise that a galvanic current passes through all the windings in the same direction. The middle side-tube is closed, and the disk in it does no service in these experiments. If now an electromotor which emits alternating currents of equal intensity and short duration be inserted between *c* and *f*, it is evident that the magnetometer-needle must continue in its position of equilibrium if the water in the glass tube is at rest, because the actions of the currents neutralize one another; but when the water is set in motion, then, because this motion alters the resistances, that current which has the same direction as the water is augmented, and the other diminished, and on this account the needle must make a deflection.

In the experiments which were made on the present occasion, induction-currents were not employed, as in Kohlrausch and Nippoldt's investigation, but an arrangement of the following nature. A circular disk of boxwood 75 millims. in diameter, and 20 millims. in thickness is covered on each side with a brass disk. By aid of a winch and cog-wheel the disk can be turned on its axis, making five turns during one revolution of the winch-handle. Fig. 2 represents a portion of the circumference of the disk. From the two brass

disks A and B little brass plates *a*, *b*, *c*, *d*, and *e* stand out, all perfectly equal in width, and situated at equal distances one from the other. They are let into the wood so that their surface coincides with the periphery of the disk. Every other of these brass plates is in connexion with the brass disk A, and every other in connexion with B. The number of these plates is 24, of which, therefore, 12 are in conducting-union with A, and the other 12 with B. The distance of the plates from one another is equal to their width. By a suitable arrangement the brass disk A was connected with one pole of an electromotor, and B with the other pole. On the periphery of the boxwood disk, two brass springs, tipped with steel, insulated from each other, slide in such a way that they are simultaneously in contact with two neighbouring brass plates. At the same time, therefore, the one is in contact with the plate *a*, and the other with plate *b*. One of these brass springs was connected with the polar disk in the upper side-tube (fig. 1); from the disk in the lower side-tube *e* a conducting-wire went to one end of the windings of the magnetometer; from the

Fig. 2.



plane. The deflections became now considerably greater, and, besides, corresponded perfectly with those obtained with the magnetometer.

The two mutually independent methods of investigation have thus conducted us to the same result, that foretold by the theory—namely, that *the resistance to galvanic conduction is diminished when the conductor and the current move in the same direction, but is increased when the directions are opposite.*

I will only mention, in conclusion, that I have tried to employ for this investigation a saturated solution of sulphate of copper, with copper poles, and a solution of sulphate of zinc, with amalgamated zinc poles; but these liquids were quite inapplicable, because I could never succeed in rendering the polar plates galvanically so equal that, when the plates dipped into the liquid, a slight current would not arise, which immediately varied as soon as the liquid was set in motion. Whence this originated I cannot say. The sulphate-of-zinc solution, however, contained some iron.

XIV. *On the Distribution of Energy in a mass of Liquid in a state of Steady Motion.* By JAMES H. COTTERILL, M.A., Professor of Applied Mechanics in the Royal Naval College, Greenwich*.

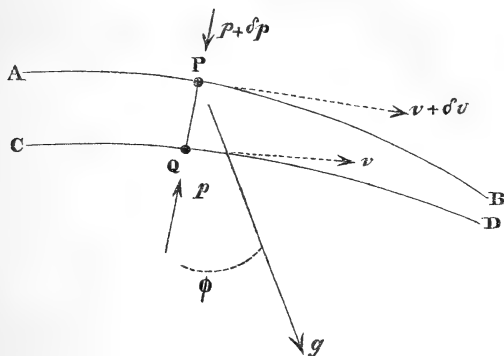
WHEN a liquid is in a state of steady motion, we know that the total energy of any given particle and of all that follow it in one and the same line of motion is a constant quantity; or, as we may otherwise express it, the total energy of each elementary stream is uniformly distributed. I am not, however, aware that it has been hitherto noticed that the distribution of energy among the several elementary streams, of which the whole current is supposed to be made up, depends solely on the molecular rotation at each point of the liquid, and is uniform when the motion is irrotational.

For simplicity suppose the motion to be in two dimensions, and to take place either in a vertical or horizontal plane. In the first case the liquid will be under the action of gravity only; and in the second, which will be included in the first, the effect of gravity need not be considered. In the figure, let A B, C D be consecutive lines of motion, and P Q a normal to these lines, and let P and Q be particles moving in these lines.} Then, if h be the total head at Q (that is, the total energy of Q per unit of weight),

$$h = z + \frac{p}{w} + \frac{v^2}{2g},$$

* Communicated by the Author.

where z is the elevation of Q above some given datum level, and w is the weight of a cubic foot of water. Differentiating,



we get

$$\delta h = \delta z + \frac{\delta p}{w} + \frac{v \delta v}{g}$$

for the head at P , where δz , being the elevation of P above Q , is given by

$$\delta z = PQ \cdot \cos \phi,$$

where ϕ is the angle PQ makes with the vertical.

But if we imagine a small cylinder described round PQ as an axis and consider its equilibrium, it is clear that

$$\delta p \cdot \alpha = \frac{w}{g} \cdot \frac{v^2}{\rho} \alpha \cdot PQ - w \cdot \alpha \cdot PQ \cdot \cos \phi,$$

where α is the sectional area of the cylinder, and ρ the radius of curvature of the lines of motion at PQ . Combining this with the former equation, we get

$$\delta h = \frac{v^2}{g\rho} \cdot PQ + \frac{v \cdot \delta v}{g} = \frac{v \cdot PQ}{g} \left\{ \frac{v}{\rho} + \frac{\delta v}{PQ} \right\}.$$

Now it is already known that, if through a given particle A lines be drawn through B and C , two particles very near to A , such that AB and AC are at right angles to each other at the instant considered, then the mean angular velocity of these lines is the same in whatever direction they be drawn through A , and is equal to the angular velocity with which a small cylindrical element described round A would rotate if supposed suddenly solidified, which mean angular velocity may hence conveniently be called the molecular rotation. In the present

case $\frac{v}{\rho}$ is the angular velocity of the tangent, that is, of a line

drawn through two consecutive moving particles ; and $\frac{\delta v}{PQ}$ is the angular velocity reckoned in the same direction of a line drawn perpendicular to the tangent through P and Q, two consecutive moving particles. The sum of the two is therefore twice the molecular rotation ; and if we call the molecular rotation ω , we shall have

$$\delta h = 2 \cdot \frac{v \cdot PQ}{g} \cdot \omega. \quad . \quad . \quad . \quad . \quad . \quad (1)$$

Now $v \cdot PQ$ is constant, being the flow in an elementary stream of breadth unity ; and hence we see that *the difference of energy between two consecutive elementary streams is proportional to the molecular rotation at any point of either.*

An immediate consequence of this is that the molecular rotation is the same at all points situated in a line of motion, and can be determined when the difference of head is known. Thus in the well-known trochoidal motion, the difference of head between one trochoidal layer and the next consecutive is easily seen to be $\frac{2r}{R} \cdot \delta r$, where r is the length of the tracing-arm, and R the radius of the rolling circle. Hence, placing this in equation (1), the known value of the molecular rotation is more easily determined than in any other way.

Again, we learn that if the energy of any portion of the liquid be initially uniformly distributed, the motion of that portion must be irrotational and must always remain so ; and thus we have a simple demonstration that the permanent motion of a perfect liquid past a solid free from discontinuity of curvature and perfectly smooth is necessarily irrotational. This demonstration clearly also applies to any case in which a perfectly smooth and fair-formed solid moves with a uniform motion of translation through a liquid at rest.

The peculiar form in which the condition for irrotational motion is expressed, namely

$$\frac{v}{\rho} + \frac{\delta v}{PQ} = 0,$$

leads to a conclusion of much importance—namely, that by a surface of “fair” form is to be understood a surface free from discontinuity of *curvature*, and not merely from discontinuity of form : thus a circular arc joined to a straight line is not a fair form, since the radius of curvature changes abruptly and occasions a discontinuity of form in the next consecutive

stream-line. This, of course, is also true when the molecular rotation is not zero, but any constant.

When the motion is not confined to one plane, the energy of the fluid will be uniformly distributed in any case in which the molecular rotation is zero, or more generally when the axis of rotation of a fluid element is likewise the direction of its motion of translation ; but the consideration of this kind of motion is postponed for the present.

XV. *The History of Young's Discovery of his Theory of Colours.* By ALFRED M. MAYER.

To the Editors of the Philosophical Magazine and Journal.

Stevens Institute of Technology, Hoboken,
New Jersey, December 13, 1875.

GENTLEMEN,

I HAVE the honour to send you for publication in your Journal the accompanying paper, entitled "The History of Young's Discovery of his Theory of Colours." Disjointed extracts from this paper have appeared in English journals; and I am therefore desirous that the entire paper should appear in your Journal, if you should deem it worthy of that distinction.

I remain, Gentlemen,

Very respectfully yours,

ALFRED M. MAYER.

The object of this communication is twofold : I desire first to give complete abstracts from the writings of Newton, Young, and Wollaston, in order to put the student of science in possession of all of the early literature relating to Young's theory of colours ; in the second place, I propose to trace the curious-history of the steps by which Young was led to the final adoption of what is now known as Young's theory of colour-sensation. In accomplishing the first of these objects, I shall at the same time attempt to show, 1st, that Young first formed an hypothesis similar to that known as Brewster's (that is, he selected red, yellow, and blue as the three simple colour-sensations); 2nd, that he subsequently modified his hypothesis and adopted red, green, and violet as the three elementary colour-sensations, showing that up to the date of this change of opinion all of his ideas on the subject were hypothetical, and not based on any observations or experiments of his own or of others ; 3rd, that this change of opinion as to the three elementary colours was made on the basis of a misconception by Wollaston of the nature of his celebrated obser-

vation of the dark lines in the solar spectrum, and also on the basis of an erroneous observation made by Young in repeating Wollaston's experiment; 4th, that Young subsequently tested his hypothesis of colour-sensation and found that it was in accord with facts reached by experiment, and that these experiments then vindicated his hypothesis and raised it to the dignity of a theory.

Before discussing the subject proper of this article, it may be well to give the reader a clear conception of Young's theory of colour-sensations, and to show in what high estimation it is at present held by men of science. This can best be done by the reading of the following short extracts from Helmholtz's 'Physiological Optics' and from his 'Popular Scientific Lectures.'

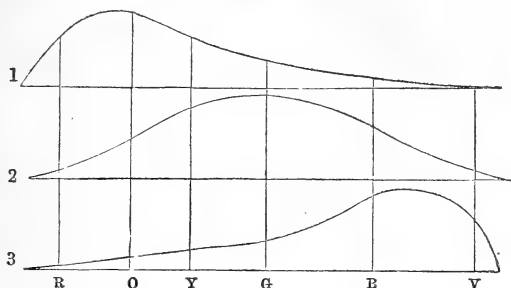
"To speak of three fundamental colours in an objective sense would be nonsense; in fact, as long as one refers only to purely physical conditions, and while there is no reference to the human eye, the properties of compound light depend alone on the proportions in which exist lights of different wave-lengths. The reduction of light to three fundamental colours can never have any thing else than a subjective signification; it consists simply in reducing all *colour-sensations* to three fundamental sensations. It is in this sense that Young understood the problem; and his hypothesis gives, in fact, an exceedingly clear and simple explanation and summary of all of the phenomena found in the physiological study of colours. Young states that—

"1. There exist in the eye three kinds of nerve-fibres, whose excitation respectively gives the sensation of red, of green, and of violet.

"2. Homogeneous light excites the three kinds of nerve-fibres with an intensity which varies with its wave-length.' That which possesses the greatest length of wave excites most powerfully the fibres sensitive to red, that which has an average wave-length excites the nerves sensitive to green, while that light formed of the shortest wave acts on the fibres which give the violet sensation. Nevertheless we cannot deny, but rather should admit for the explanation of numerous phenomena, that each colour of the spectrum excites all three kinds of nerve-fibres, but with different intensities. Imagine the colours of the spectrum arranged horizontally, going from the red, R, to the violet, V, as shown at the base of fig. 1. The three curves will then represent more or less exactly the degrees of irritability of the three kinds of nerve-fibres (1, the red; 2, the green; 3, the violet) for the various colours of the spectrum.

“Pure red excites strongly the fibres sensitive to red, and feebly the two other kinds of fibres ; sensation, red.

Fig. 1.



“Pure yellow excites moderately the fibres sensitive to red and to green, feebly those fibres sensitive to violet ; sensation, yellow.

“Pure green excites strongly the fibres sensitive to green, feebly those sensitive to red and to violet ; sensation, green.

“Pure blue excites in a moderate degree those fibres sensitive to green and to violet, feebly those sensitive to red ; sensation, blue.

“Pure violet excites strongly those fibres specially destined to receive this sensation, and the other fibres are feebly affected by this light ; sensation, violet.

“The nearly equal excitation of all of the fibres will give the sensation of white, or of whitish colours.

“ The choice of the three fundamental colours is to some extent arbitrary. We can choose at will any three colours whose mixture produces white. *Young no doubt was guided by the consideration that the extreme colours of the spectrum occupied the privileged positions**. If we do not choose these colours, we must take for one of the colours a purple tint, and the curve which responds to it in the figure will have two maxima—one in the red, the other in the violet. The hypothesis, without being an impossible one, will be more complicated. As far as I know of, there exists no means of determining directly the fundamental colours but the examination of persons affected with colour-blindness. We will subsequently see how far that examination confirms the hypothesis of Young, at least so far as the red is concerned.

“ In general, then, light which consists of undulations of different wave-lengths produces different impressions upon

* The writer has italicized the above for the purpose of a future reference to it.

our eye, namely those of different colours. But the number of hues which we can recognize is much smaller than that of the various possible combinations of rays with different wavelengths which external objects can convey to our eyes. The retina cannot distinguish between the white which is produced by the union of scarlet and bluish-green light, and that which is composed of yellowish green and violet, or of yellow and ultramarine-blue, or of red, green, and violet, or of all the colours of the spectrum united. All these combinations *appear* identically as white; and yet from a *physical point of view* they are very different. In fact the only resemblance between the several combinations just mentioned is, that they are indistinguishable to the human eye. For instance, a surface illuminated with red and bluish-green light would come out black in a photograph; while another lighted with yellowish green and violet would appear very bright, although both surfaces alike seem to the eye to be simply white.

“ Other colours also, especially when they are not strongly pronounced, may, like pure white light, be composed of very different mixtures, and yet appear indistinguishable to the eye, while in every other property, physical or chemical, they are entirely distinct.

“ The theory of colours, with all these marvellous and complicated relations, was a riddle which Goethe in vain attempted to solve; nor were we physicists and physiologists more successful. I include myself in the number; for I long toiled at the task without getting any nearer my object, until I at last discovered that a wonderfully simple solution had been discovered at the beginning of this century, and had been in print ever since for any one to read who chose. This solution was found and published by the same Thomas Young who first showed the right method of arriving at the interpretation of Egyptian hieroglyphics. He was one of the most acute men who ever lived, but had the misfortune to be too far in advance of his contemporaries. They looked on him with astonishment, but could not follow his bold speculations; and thus a mass of his most important thoughts remained buried and forgotten in ‘The Transactions of the Royal Society,’ until a later generation by slow degrees arrived at the rediscovery of his discoveries, and came to appreciate the force of his argument and the accuracy of his conclusions.”

The first publication by Young of his theory of colour appeared in a Bakerian Lecture, entitled “On the Theory of Light and Colours,” which Young read before the Royal Society on November 12, 1801. In the opening part of this lecture he says:—“A more extensive examination of Newton's

various writings has shown me that he was in reality the first who suggested such a theory [*the undulatory theory of light*], as I shall endeavour to maintain; that his own opinions varied less from this theory than is now almost universally supposed; and that a variety of arguments have been advanced, as if to confute him, which may be found in a similar form in his own works; and this by no less a mathematician than Leonard Euler, whose system of light, as far as it is worthy of notice, either was, or might have been, wholly borrowed from Newton, Hooke, Huyghens, and Malebranche.

“Those who are attached, as they may be with the greatest justice, to every doctrine which is stamped with the Newtonian approbation, will probably be disposed to bestow on these considerations so much the more of their attention as they shall appear to coincide more nearly with Newton’s opinion. For this reason, after having briefly stated each particular position of my theory, I shall collect from Newton’s various writings such passages as seem to be the most favourable to its admission; and although I shall quote some papers which may be thought to have been partly retracted at the publication of the *Optics*, yet I shall borrow nothing from them that can be supposed to militate against his maturer judgment.”

The fact that Young, the *founder* of the undulatory theory of light, in this Bakerian Lecture, in which it has been said that he laid the foundations of that doctrine, should set forth his views in a series of postulates followed by citations from the writings of Newton to give them weight and proof, may justly surprise those who have trusted to the second-hand information derived from carelessly compiled text-books and from hastily prepared popular lectures. But then where would be the pugilistic charm of the popular lecturer on the undulatory theory of light if Newton, his champion, the violent defender of the emanation cause, should decline to enter as a contestant?

Under the heading of Hypothesis III. of this paper we first meet Young’s theory of colour-sensation.

“Hypothesis III. *The Sensation of different Colours depends on the different frequency of Vibrations, excited by Light in the Retina.*

Passages from Newton.

“The objector’s hypothesis, as to the fundamental part of it, is not against me. That fundamental supposition is, that the parts of bodies, when briskly agitated, do excite vibrations in the ether, which are propagated every way from those bodies in straight lines, and cause a sensation of light, by beating and dashing against the bottom of the eye, something after

the manner that vibrations in the air cause a sensation of sound, by beating against the organs of hearing. Now the most free and natural application of this hypothesis to the solution of phenomena I take to be this: that the agitated parts of bodies, according to their several sizes, figures, and motions, do excite vibrations in the ether of various depths or bignesses, which being promiscuously propagated through that medium to our eyes, effect in us a sensation of light of a white colour; but if by any means those of unequal bigness be separated from one another, the largest beget a sensation of a red colour, the least or shortest of a deep violet, and the intermediate ones of intermediate colours; much after the manner that bodies, according to their several sizes, shapes, and motions, excite vibrations in the air, of various bignesses, which, according to those bignesses, make several tones in a sound; that the largest vibrations are best able to overcome the resistance of a refracting superficies, and so break through it with least refraction; whence the vibrations of several bignesses (that is, the rays of several colours which are blended together in light) must be parted from one another by refraction, and so cause the phenomena of prisms and other refracting substances; and that it depends upon the thickness of a thin transparent plate or bubble whether a vibration shall be reflected at its further superficies or transmitted; so that, according to the number of vibrations interceding the two superficies, they may be reflected or transmitted for many thicknesses. And since the vibrations which make blue and violet are supposed shorter than those which make red and yellow, they must be reflected at a less thickness of the plate; which is sufficient to explicate all the ordinary phenomena of those plates or bubbles, and also of all natural bodies whose parts are like so many fragments of such plates. These seem to be most plain, genuine, and necessary conditions of this hypothesis. And they agree so justly with my theory, that if the animadversor think fit to apply them, he need not, on that account, apprehend a divorce from it. But yet, how he will defend it from other difficulties, I know not." (Phil. Trans. vol. vii. p. 5088; Abr. I. 145, November 1672.)

"To explain colours, I suppose that as bodies of various sizes, densities, or sensations do, by percussion or other action, excite sounds of various tones, and consequently vibrations on the air of different bigness, so the rays of light, by impinging on the stiff refracting superficies, excite vibrations in the ether—of various bigness; the biggest, strongest, or most potent rays the largest vibrations; and others shorter, according to their bigness, strength, or power; and therefore the ends of

the capillamenta of the optic nerve, which pave or face the retina, being such refracting superficies, when the rays impinge upon them, they must there excite these vibrations, which vibrations (like those of sound in a trunk or trumpet) will run along the aqueous pores or crystalline pith of the capillamenta, through the optic nerve into the sensorium; and there, I suppose, affect the sense with various colours, according to their bigness and mixture; the biggest with the strongest colours, reds and yellows; the least with the weakest, blues and violets; the middle with green; and a confusion of all with white, much after the manner that, in the sense of hearing, nature makes use of aerial vibrations of several bignesses, to generate sounds of divers tones; for the analogy of nature is to be observed." (Birch, vol. iii. p. 262, December 1675.)

"Considering the lastingness of the motions excited in the bottom of the eye by light, are they not of a vibrating nature? Do not the most refrangible rays excite the shortest vibrations,—the least refrangible the largest? May not the harmony and discord of colours arise from the proportions of the vibrations propagated through the fibres of the optic nerve into the brain, as the harmony and discord of sound arise from the proportions of the vibrations of the air?" (Optics, Qu. 16, 13, 14.)

After these quotations from Newton, Young brings out his hypothesis of colour-sensations under the following

"*Scholium.* Since, for the reason here assigned by Newton, it is probable that the motion of the retina is rather of a vibratory than of an undulatory nature, the frequency of the vibrations must be dependent on the constitution of this substance. Now, as it is almost impossible to conceive each sensitive point of the retina to contain an infinite number of particles, each capable of vibrating in perfect unison with every possible undulation, it becomes necessary to suppose the number limited; for instance, to the three principal colours, red, yellow, and blue, of which the undulations are related in magnitude nearly as the numbers 8, 7, and 6; and that each of the particles is capable of being put in motion, less or more forcibly, by undulations differing less or more from a perfect unison; for instance, the undulations of green light, being nearly in the ratio of $6\frac{1}{2}$, will affect equally the particles in unison with yellow and blue, and produce the same effect as a light composed of those two species; and each sensitive filament of the nerve may consist of three portions, one for each principal colour."

An attentive perusal of the above quotation will show that Young's hypothesis imagines each sensitive point of the retina to contain *particles* capable of vibrating in perfect unison to those vibrations causing three principal colours (red, yellow,

and blue, in this the first publication of his *hypothesis*), "and that each of the particles is capable of being put in motion, less or more forcibly, by undulations differing less or more from a perfect unison." This would suppose such a triple *molecular* constitution of each nerve-fibril as to cause the three species of its constituent molecules (or the atoms forming the molecules) to be *in tune* with the three rates of vibration corresponding respectively to the undulations of the æther causing red, yellow, and blue. He afterwards says, "and each sensitive filament of the nerve may consist of three portions, one for each principal colour." We have here a conception of the *mode of action* of an ætherial vibration on the retinal nerve-fibril which has not been described by those who have given accounts of Young's theory of colour-sensation. Before reading the celebrated Bakerian Lecture, the only knowledge I had directly obtained of Young's theory was from the reading of the account of it as published in vol. i. page 439 of his 'Lectures on Natural Philosophy and the Mechanical Arts,' London, 1807. This account, however, contains no mention of the physiological part of his theory; and last May I published in the Philosophical Magazine my paper No. 6 of "Researches in Acoustics," in which (p. 363) I expressed similar views to those just quoted from Young, as follows:—

"For, has modern histology given us any facts concerning the structure of the human retina which point to the establishment of Young's hypothesis of three distinct sets of retinal nerve terminations? The more we study the minute structure of the retinal rods and cones, the further appears to remove an understanding of the mode of operation of the sensory apparatus of the eye. May not research in this direction be guided by the hypothesis that the *molecular* constitution of the retinal rods and cones is such that their molecules are severally tuned to the vibrations corresponding to the colours red, green, and violet? This would lead us to look for effects of actinism on the retina as showing the link existing between the transmitting and sensory functions of the eye. Do not the facts of the known persistence of chemical action, after it has been once initiated, and the time which would be required for the retinal molecules to recombine or rearrange themselves after the ætherial vibrations had ceased, comport with the known durations of the residual visual sensations, and with the main facts of physiological optics, better than the hypothesis that masses of the retinal elements are set in vibration rather than their molecules?"

It requires no argument, it is evident, that the statements made by Young in the foregoing paper, concerning his colour

hypothesis, were entirely hypothetical, not having been based on any observation or experiment either of his own or of others.

The next publication by Young on his theory of colour takes place in the following year, and is contained in the following short paragraph, incidentally written toward the conclusion of a paper read by him before the Royal Society on July 1, 1802; and entitled "An account of some cases of the production of colours, not hitherto described."

"In consequence of Dr. Wollaston's correction of the description of the prismatic spectrum, compared with these observations, it becomes necessary to modify the supposition that I advanced in the last Bakerian Lecture, respecting the proportions of the sympathetic fibres of the retina; substituting red, green, and violet for red, yellow, and blue, and the numbers 7, 6, and 5 for 8, 7, and 6."

It thus appears that Young changed his three elementary colour-sensations from red, yellow, and blue to *red, green, and violet*, "in consequence of Dr. Wollaston's correction of the description of the prismatic spectrum." In order to understand fully the ground for this modification of his hypothesis, one will be obliged to read the following abstract from Dr. Wollaston's paper, published in the 'Transactions of the Royal Society' for 1802; and we are pleased to detain the reader with this paper, because it contains the first publication of the observation of those dark lines to which modern spectroscopic research has given such important and prominent significance.

"I cannot conclude these observations on dispersion without remarking that the colours into which a beam of white light is separable by refraction, appear to me to be neither 7, as they usually are seen in the rainbow, nor reducible by any means (that I can find) to 3, as some persons have conceived; but that, by employing a very narrow pencil of light, four primary divisions of the prismatic spectrum may be seen, with a degree of distinctness that, I believe, has not been described nor observed before.

"If a beam of daylight be admitted into a dark room by a crevice $\frac{1}{20}$ of an inch broad, and received by the eye at the distance of 10 to 12 feet through a prism of flint glass, *free from veins*, held near the eye, the beam is seen to be separated into the four following colours only, red, yellowish green, blue, and violet, in the proportions represented in fig. 2.

"The line A that bounds the red side of the spectrum is somewhat confused, which seems in part owing to want of power in the eye to converge red light. The line B, between red and green, in a certain position of the prism, is perfectly distinct; so also are D and E, the two limits of violet. But

C, the limit of green and blue, is not so clearly marked as the rest ; and there are also, on each side of this limit, other distinct

Fig. 3.



dark lines, *f* and *g*, either of which, in an imperfect experiment, might be mistaken for the boundary of these colours.

“The position of the prism in which the colours are most clearly divided is when the incident light makes about equal angles with two of its sides. I then found that the spaces A B, B C, C D, D E, occupied by them, were nearly as the numbers 16, 23, 36, 25.”

In the light of the subsequent careful examinations of the spectrum made in 1814–15 by the celebrated optician Fraunhofer of Munich, we can ascertain what Wollaston really saw. Wollaston, in short, only observed *imperfectly* the dark lines of the spectrum, now known as Fraunhofer's lines ; but he *imagined* he saw a spectrum so pure (that is, formed by such a degree of dispersion) that it became divided into four distinct and separated “primary divisions.” He at once inferred, and erroneously, that Newton's analysis of the sun's light was false—that no *orange* or *yellow* exists in the spectrum, but between the red and the blue there exists only a “yellowish green.” Further on we shall see how Young made a similar but even greater error in his description of this observation. I imagine that when Wollaston's sharp eye caught the glimpse of the divided spectrum, he naturally thought he saw in those divisions uniform colours. It was a natural mistake, and only too readily made, by reason of his mind imagining that the dark lines were the *dividing lines* of the pure simple colours of the solar spectrum.

In the figure illustrating Wollaston's observation, I have placed alongside of his lines A, B, *f*, *g*, D, and E the corresponding Fraunhofer letters B, D, *b*, F, G, H, thus giving the reader a clear idea of what Wollaston really saw. Wollaston's line “B, between the red and the green,” and which he says “in a certain position of the prism is perfectly distinct,”

we at once recognize as the dark solar line D, occupying a position between the orange and yellow, and well known to be caused by the reversal of the bright yellow light of sodium vapour. No one, however, could now say, after an examination of the spectrum as observed by Wollaston, that the line D divides the red from the green. Wollaston also calls his D and E lines (the G and H lines of Fraunhofer) "the two limits of the violet;" we now know that G is really on the indigo, and that H is within the limits of the violet.

For a more satisfactory comparison of the colours of the solar spectrum as observed by Wollaston and Fraunhofer, I give below the following Table. Fraunhofer's results are taken from his coloured figure of the spectrum. Both spectra are from flint glass, and their lengths are supposed divided into 360 equal parts.

	Fraunhofer.	Wollaston.	
Red	56	57.6	Red.
Orange . . .	27		
Yellow . . .	27		
Green	46	82.8	Yellowish green.
Blue	48	129.6	Blue.
Indigo . . .	47		
Violet . . .	109	90	Violet.
	<u>360</u>	<u>360</u>	

Fraunhofer's observations are irreproachable, and are to this day in high esteem for their accuracy. They were made by placing the prism in front of a telescope mounted on a divided horizontal circle, and viewing a distant slit through the prism and telescope. He observed spectra as pure as those given by modern spectroscopes of low power. Fraunhofer discerns orange and yellow and green where Wollaston only sees yellowish green. Also Wollaston did not see all of the violet, as we might suspect from his having bounded its upper limits by the line H. Fraunhofer saw 109 parts of violet, Wollaston only 90.

The above discussion, I think, has clearly shown that Wollaston made a false interpretation of his observation in supposing that he had discovered a pure spectrum naturally divided by dark lines into four simple colours, and that he also erred in the relative proportions which he gave to them. Also I have shown that Young, in finally selecting red, green, and violet as the three elementary colour-sensations, was not, as Helmholtz states, guided in their choice "by the consideration that the extreme colours of the spectrum occupied the privileged positions," but selected those colours on hearing of Wol-

laston's supposed complete analysis of the sun's light into red, greenish blue, and violet colours separated from each other in the spectrum by dark spaces.

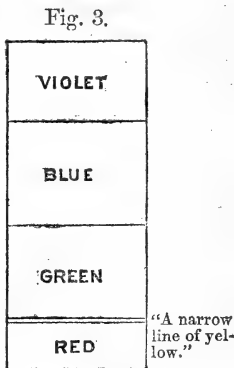
We hear no more from Young about his theory of colours until 1807, when he published the first volume of his celebrated work, 'A Course of Lectures on Natural Philosophy and the Mechanical Arts'*. On page 439 *et seq.* of this work Young gives a concise statement of his views on the analysis of the sensations of colour, and supports these views with conclusive experiments with rotating coloured disks; but, strange to say, he omits from this account of his theory all mention of the physiological explanation of it which he gave in the Bakerian Lecture of 1801. The following extracts from the 'Natural Philosophy' give all that it contains on the theory of colours. The italics are our own.

"It has generally been supposed, since the time of Newton, that when the rays of light are separated as completely as possible by means of refraction, they exhibit seven varieties of colour related to each other with respect to the extent that they occupy in ratios nearly analogous to those of the ascending scale of the minor mode in music. The observations were, however, imperfect, and the analogy was wholly imaginary. Dr. Wollaston has determined the division of the coloured image or spectrum in a much more accurate manner than had been done before. By looking through a prism at a narrow line of light, he produces a more effectual separation of the colours than can be obtained by the common method of throwing the sun's image on a wall. The spectrum formed in this manner *consists of four colours only, red, green, blue and violet*, which occupy spaces in the proportion of 16, 23, 36, and 25 respectively, making together 100 for the whole length, the red being nearly one sixth, the green and the violet each about one fourth, and the blue more than one third of the length. *The colours differ scarcely at all in quality within their respective limits; but they vary in brightness*, the greatest intensity of light being in that part of the green which is nearest the red. *A narrow line of yellow is generally visible at the limit of the red and green; but its breadth scarcely exceeds that of the aperture by which the light is admitted, and Dr. Wollaston attributes it to the mixture of the red with the green light.* There are also several

* "We have heard it remarked," says Dean Peacock in his 'Life of Young, "that no writer, on any branch of science which the lectures treat of, can safely neglect to consult them, so rich is the mine of knowledge which they contain; and it is a well-known fact that many important propositions and discoveries have been more or less clearly indicated in them, which have only been recognized or pointed out when other philosophers discovered them independently, or announced them as their own.'

dark lines crossing the spectrum within the blue portion and in its neighbourhood, in which the continuity of the light seems to be interrupted. This distribution of the spectrum Dr. Wollaston has found to be the same, whatever refracting substance may have been employed for its formation; and he attributes the difference which has sometimes been observed in the proportions to accidental variations of the obliquity of the rays. The angular extent of the spectrum formed by a prism of crown glass is $\frac{1}{27}$ of the deviation of the red rays, by a prism of flint glass $\frac{1}{19}$ " (fig. 3)*.

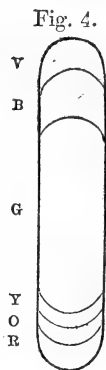
Fig. 3. "The spectrum produced by looking through a prism at a narrow line of light.



"In light produced by the combustion of terrestrial substances the spectrum is still more interrupted; thus the bluish light of the lower part of the flame of a candle is separated by refraction into five parcels of various colours; the light of burning spirits, which appears perfectly blue, is chiefly composed of green and violet rays; and the light of a candle into which salt is thrown abounds with a *pure yellow inclining to green*, but not separable by refraction. The electric spark furnishes also a light which is differently divided in different circumstances.

"If the breadth of the aperture viewed through a prism is somewhat increased, the space occupied by each variety of light in the spectrum is augmented in the same proportion, and each portion encroaches on the neighbouring colours and is mixed with them; so that the red is succeeded by orange, yellow, and yellowish green, and the blue is mixed on the one side with the green, and on the other with the violet; and it is in this state that the prismatic spectrum is commonly exhibited.

Fig. 4. "The appearance of a circular aperture, moderately large, viewed through a prism.



"Sir Isaac Newton observed that the effect of white light

* Figures 3 and 4 are copied of the exact size of those given by Young in the plates appended to his 'Natural Philosophy.' The descriptions of the figures are those given by Young. The colours in Young's figures we have indicated in type. There are six other figures illustrating

on the sense of sight might be imitated by a mixture of colours taken from different parts of the spectrum, notwithstanding the omission of some of the rays naturally belonging to white light. Thus if we intercept one half of each of the four principal portions into which the spectrum is divided, the remaining halves will still preserve, when mixed together, the appearance of whiteness; so that it is probable that the different parts of those portions of the spectrum which appear of one colour have precisely the same effect on the eye. It is certain that the perfect sensations of yellow and of blue are produced respectively by mixtures of red and green and of green and violet light; and there is reason to suspect that those sensations are always compounded of the separate sensations combined; at least this supposition simplifies the theory of colours; it may therefore be adopted with advantage until it be found inconsistent with any of the phenomena; and we may consider white light as composed of a mixture of red, green, and violet only, in the proportion of about two parts red, four green, and one violet, with respect to the quantity or intensity of the sensations produced.

“If we mix together in proper proportions any substances exhibiting these colours in their greatest purity and place the mixture in a light sufficiently strong, we obtain the appearance of perfect whiteness; but in a fainter light the mixture is grey, or of that hue which arises from a combination of white and black, black bodies being such as reflect white light, but in a very scanty proportion. For the same reason, green and red substances mixed together usually make rather a brown than a yellow colour, and many yellow colours, when laid on very thickly or mixed with black, become brown. The sensations of various kinds of light may also be combined in a still more satisfactory manner by painting the surface of a circle with different colours in any way that may be desired, and causing it to revolve with such rapidity that the whole may assume the appearance of a single tint, or a combination of tints resulting from the mixture of the colours.

“From three simple sensations, with their combinations, we obtain seven primitive distinctions of colours; but the different proportions in which they may be combined afford a variety of tints beyond all calculation. The three simple sensations being red, green, and violet, the three binary combinations are yellow, consisting of red and green; crimson, of red and violet; and blue, of green and violet; and the seventh in order is white light composed of all three united. But the blue thus

Young's account; but we have found it impossible to convey in *print* clear ideas of their tints.

produced by combining the whole of the green and violet rays is not the blue of the spectrum; for four parts of green and one of violet make a blue differing very little from green; while the blue of the spectrum appears to contain as much violet as green; and it is for this reason that red and blue usually make a purple, deriving its hue from the predominance of violet.

“It would be possible to exhibit at once to the eye the combinations of any three colours in all imaginable varieties. Two of them might be laid down on a revolving surface in the form of triangles placed in opposite directions, and the third on projections perpendicular to the surface, which, while the eye remained at rest in any one point obliquely situated, would exhibit more or less of their painted sides as they passed through their different angular positions: and the only further alteration that could be produced in any of the tints would be derived from the different degrees of light only. The same effect may also be exhibited by mixing the colours in different proportions by means of the pencil, beginning from three equidistant points as the centres of the respective colours.”

On certain portions of the above extracts, which I have italicized, I will venture a few observations. In the first passage thus indicated, Young says:—“The *spectrum formed in this manner consists of four colours only, red, green, blue, and violet.*” Young here replaces Wollaston’s “yellowish green” by “green;” and further on he adds, “The colours differ scarcely at all in quality within their respective limits; but they vary in brightness, the greatest intensity of light being in that part of the green which is nearest to the red,” thus confirming our view that both Young and Wollaston were of the opinion that, when a narrow bright crevice is observed through a prism, the spectrum so viewed consists of only four colours, red, green, blue, and violet, “differing scarcely at all in quality within their respective limits;” which limits they supposed naturally existed in the dark spaces which, as they imagined, bounded these elementary colours. Young, however, somewhat modifies this opinion in the next sentence, when he says, “A narrow line of yellow is generally visible at the limit of the red and green; but its breadth scarcely exceeds that of the aperture by which the light is admitted, and Dr. Wollaston attributes it to the mixture of the red with the green light.” It would indeed appear from the last portion of this sentence that Young obtained directly from Wollaston one of the main facts on which his theory was founded, namely that yellow can be reproduced by the mixture of red and green lights. But Wollaston, in his paper of 1802, from which we have cited, makes no such

statement as to the composition of yellow light; and it is therefore probable that Wollaston communicated orally this view of the subject to Young. Every student of optics now knows that the description already given of Fraunhofer's observation on the colour composition of the spectrum is the correct one; yet the errors of observation of Wollaston and of Young were errors which led to a great discovery, as we shall see on the further examination of the history of this beautiful and comprehensive theory of colour.

Further on in the 'Natural Philosophy' we read that "the sensations of various kinds of light may also be combined in a still more satisfactory manner by painting the surface of a circle with different colours, in any way that may be desired, and causing it to revolve with such rapidity that the whole may assume the appearance of a single tint, or of a combination of tints, resulting from the mixture of the colours." These experiments were evidently first made by Young, and are fully described in the text and perfectly illustrated in the coloured disks in the plates of Young's work. These experiments have been carefully repeated by Helmholtz, Maxwell, and others; and of their general accuracy there is no doubt. We can readily imagine the delight with which Young must have viewed these beautiful experiments, which, however, together with other truths unfolded by him, were destined to remain unnoticed "until a later generation, by slow degrees, arrived at the *discovery* of his discovery."

It must now recur to the reader to inquire *when* were made these experiments which first confirmed Young's hypothesis and placed it among the best-established truths of optical science, and why it was that Young should for so long a time have been satisfied with a hypothetical statement of his views on the colour-sensations, and should have deferred to bring those views to the test of experiment. For reasons already stated, Young, in July 1802, changed his three elementary colour-sensations, red, yellow, and blue, to red, green, and violet. The experiments with the rotating coloured disks were first published in 1807. Young printed the syllabus of his first course of lectures on January 19, 1802, in a volume of 250 pages. I have not been able to procure a copy of this syllabus; but evidently it does not contain even the corrected statement of his theory of colour; for that was based on Wollaston's observations, which appeared subsequently to the syllabus, on June 24, 1802*. It is therefore evident that,

* Professor Tyndall has recently informed me that Young's syllabus gives red, yellow, and blue as the three elementary colour-sensations.

unless Young made the experiments with the rotating coloured disks during the latter part of his course of lectures, he must have made them during the interval between his retirement from the professorship at the Royal Institution and the publication of his lectures on Natural Philosophy in 1807. Young delivered his first lecture before the Royal Institution on January 20, 1802, and was very busy with his lectures until July 4, 1803, when he retired. I think that we may fix the date of these remarkable experiments as somewhere between 1803 and 1807; and it is highly probable that the theory was never given to the public in a lecture before the Royal Institution, but first appeared in the publication of his Lectures on Natural Philosophy.

That Young should have delayed to bring to the test of experiment a plausible hypothesis, when other men would at once have appealed to the instruments in their laboratories, is explained by the fact that Young "at no period of his life was fond of repeating experiments or even of originating new ones. He considered that, however necessary to the advancement of science, they demanded a great sacrifice of time, and that, when a fact was once established, that time was better employed in considering the purposes to which it might be applied or the principles which it might tend to elucidate." Indeed this peculiarity receives abundant confirmation from his own words. Thus, in the Bakerian Lecture, already so often referred to, he says:—"Nor is it absolutely necessary in this instance [in speaking of the proofs to be adduced in support of the undulatory theory of light] to produce a single new experiment; for of experiments there is already an ample store." And in a letter written in November 1827, to his sister-in-law, Mrs. Earle, on the respective honours given by Herschel, in his 'Optics,' to Young and Fresnel, he says, "And acute suggestion was then, and indeed always, more in the line of my ambition than experimental illustration." Young carried his opinion of the secondary importance of experiments so far as even to object to the increase of the fund left by Wollaston to the Royal Society to aid experimental inquiries, in these words:—"For my part, it is my pride and pleasure, as far as I am able, to supersede the necessity of experiments, and more especially of expensive ones."

XVI. *Contributions to the Mineralogy of Nova Scotia.* By HENRY HOW, D.C.L., Professor of Chemistry, University of King's College, Windsor, Nova Scotia.

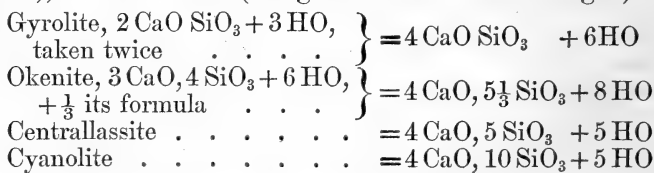
[Continued from S. 4. vol. xli. p. 274.]

VII. *Some Triassic Trap Minerals.*

CENTRALLASSITE.—The publication of a letter on “Doubtful Minerals” in the ‘Chemical News’ (vol. xxx. p. 165), has recalled my attention to some mineralogical work which occupied a good deal of my spare time in former years. In that letter, and in others subsequently written to the same Journal on the topic indicated, allusion is made to some of this work; and I open this communication with an attempt to make matters clear so far as this is concerned.

In 1859 I described (in the ‘Edinburgh New Philosophical Journal,’ vol. x. p. 84) three minerals found in trap of Triassic age near Black Rock in the Bay of Fundy, which I considered new, and which I called cyanolite, cerinite, and centrallassite: they are among the “doubtful minerals” of the letters above mentioned. I have nothing to add to what was originally given with regard to the first two of these; but I find, on referring to my notes, that I can contribute something more to the history of the third; and while preparing this paper, I have found occasion to reclaim a formula assigned to a related mineral by its discoverer, the late Professor Anderson, of Glasgow. Some notes respecting other trap minerals and the rocks in which they occur are added.

The three minerals mentioned were found constituting a nodule: cyanolite had somewhat the characters of chalcedony; centrallassite was a colourless lamellar mineral in radiated spherical concretions, and cerinite a yellowish waxy-looking mineral enveloping the others. From the analytical details obtained I thought I was justified in giving formulæ for the minerals; and as regards the first two (cerinite contained alumina), their relations, as made out, are shown by taking the same number of equivalents of lime (the base in each formula), in this manner (using the notation then in vogue):—



I did not fail to state that centrallassite came near gyrolite in physical characters and chemical composition. Dana's cri-

tical observation with reference to cyanolite is, "probably the same mineral with centrallassite, impure with much more silica; or it is chalcedony, impure with centrallasite" (*Mineralogy, Supplement to fifth edition, p. 797*). This, of course, may be the case; but if so, it is curious that the whole of it should have the same composition, as my two analyses would seem to show. In any case, I have no doubt Dr. Burghardt is incorrect when he says "cyanolite is simply okenite" (*Chemical News, vol. xxx. p. 185*).

With regard to centrallassite, it had, in all probability, many years before I came to this country attracted the attention of Dr. Abraham Gesner, who was one of the earliest collectors of minerals in this his native province, and was probably more familiar with its choice localities for trap species than any one else has ever been. My reason for saying this is, that after my paper was published I observed in his '*Remarks on the Geology and Mineralogy of Nova Scotia*,' 1836 (p. 202), the following:—"This mineral, called prehnite, found a few miles east of Black Rock, occurs in small botryoidal masses, of which none were larger than a hen's egg. It consists of very delicate crystals radiating in all directions from the centre. A number of these small circular clusters are collected together, each having its fibres proceeding from an adjusted centre to the extremity of the group composed by them; its colour is pale green, supposed to arise from the green carbonate of copper also found near it. Before the blowpipe it intumesces and melts to a spongy black enamel. It does not gelatinize, and is therefore distinguished from zeolite. It appeared in amygdaloid near its junction with superimposed basaltic trap. Locality visited next spring; no more mineral found."

Now in this extract I see an accurate description of part of the nodule described in my paper of 1869. Gesner gave no analysis; but the "intumescence before the blowpipe," while it is a character belonging to prehnite, agrees closely enough also with my statement with regard to centrallassite, "fuses readily, with continued spirting, to an opaque glassy bead." I find, on repeating the experiment, that the spirting is accompanied by intumescence. As respects the green colour, that is no doubt accidental; it is frequently seen in minerals properly colourless, in these districts; and it is caused, as Gesner supposed, by copper, as I showed in the case of laumonite found in the same range of trap at Margaretville (*Edin. New Phil. Journ. 1858*). Again, like centrallassite, his mineral did not gelatinize; and moreover the locality agrees with mine, estimated to be some very few miles east of Black Rock, as it was between this and Hall's Harbour, about eight miles

distant. Prehnite, I may mention, has never been found, to a certainty, in this province, so far as I know.

Some little time after my paper was published I obtained from Dr. Webster*, my companion when the nodule was found, a small mass of mineral precisely described in the words of Dr. Gesner above, only that there was an entire absence of colour. It was found detached upon the beach 100 yards or so from the pier at Hall's Harbour. It was about the size of a bantam's egg, weighed close upon 150 grains, and seemed to be uniform, except that there was one very small patch resembling cerinite; the major part was of high pearly lustre, and either transparent or strongly translucent; but a considerable portion of the surface was opaque. This appearance the remains of the specimen in my possession retain to this day. As it is sometimes important to record historical details in the case of "doubtful minerals," I may mention that I placed about half the piece I received from Dr. Webster in the hands of Professor Marsh, of Yale College, who at that time was a student there, and used to collect extensively in this province during his vacations; and I remember perfectly well his saying that the mineral was unfamiliar to him. An analysis was made at the time of portions partly pearly, partly opaque: the sample having been ignited for water, the residue was treated with acid, and the resulting "silica," fused with carbonates of sodium and potassium; alumina and lime were separated and added to the quantities in the original fluid. The final results on the air-dry mineral were:—

		Oxygen.
Lime	31·53	9
Alumina . . .	2·19	
Potassa . . .	0·76	
Water	11·58	10·29
Silica	54·72	29·18
	<hr/> 100·78	

These numbers, on comparison with the following mean of my former analyses and the percentages calculated from the formula to which they led,

* A well-known and successful collector, whose fine cabinet now forms a prominent feature in the Provincial Museum, Halifax, N. S., by the generosity of his widow.

Centrallassite, 1859.	
	Found. Calculated.
Lime . . .	27·91 29·20
Alumina . .	1·14
Magnesia .	0·16
Potassa . .	0·59
Water . . .	11·41 11·74
Silica . . .	58·86 59·06
	<hr/> 100·07 100·00

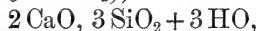
establishes beyond a doubt the identity of the mineral; but the recurrence of an almost absolutely similar quantity of water is so far counterbalanced by dissimilar amounts of the other essential constituents that I must own my old formula is not supported as I should like. Since one of the minerals associated with the specimen first analyzed contained much silica (cyanolite; it gave 74 per cent.), and was not easily separated; and as it is not present with the mineral whose analysis is now published, I think it most probable this last is the more correct. The oxygen ratio is seen to be nearly 1 : 3 : 1 for lime, silica, and water: now, taking the accepted constitution of silica and the equivalent notation still most commonly employed in mineralogical writings, this ratio leads to the formula



of which the calculated percentages—

2 CaO =	56	34·14
2 HO =	18	10·98
3 SiO ₂ =	90	54·88
	<hr/> 164	<hr/> 100·00

agree well enough with the analytical results brought forward, allowing for replacements. The ratio for the same constituents given by Anderson for gyrolite ("Description and Analysis of Gurolite, a new Mineral Species," Phil. Mag. Feb. 1851), was 2 : 6 : 3; and the percentages calculated from the formula he gave (changing SiO₃ to SiO₂),



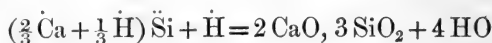
2 CaO =	56	32·37
3 HO =	27	15·61
3 SiO ₂ =	90	52·02
	<hr/> 173	<hr/> 100·00

agree tolerably well with the numbers found by him, and better with those found by myself ("On Gyrolite occurring with Calcite in Apophyllite in Trap of Bay of Fundy," Edin. New Phil. Journ. and Silliman's Journal, 1861) in the only

two recorded analyses of the mineral (in my paper allusion is made to the probable replacements), viz. :—

Gyrolite.		
	Anderson.	How.
Lime . . .	33·24	29·95
Alumina . .	1·48	1·27
Magnesia . .	0·18	0·08
Potassa . .	none	1·60
Water . . .	14·18	15·05
Silica . . .	50·70	51·90
	<hr/> 99·78	<hr/> 99·85

I wish to draw attention to this concordance particularly, because I find in Dana (Mineralogy, fifth edition, p. 398) the formula



assigned to gyrolite : the percentages calculated from this expression,

$\text{SiO}_2 = 30$. . .	49·45
$\frac{2}{3}\text{CaO} = 18·66$. . .	30·76
$1\frac{1}{3}\text{HO} = 12·00$. . .	19·78
		<hr/> 99·99

by no means correspond with our experimental results. Again, I find that Greg and Lettsom (Manual of Mineralogy of Great Britain and Ireland, p. 217) give for the same mineral the formula



with the calculated percentages

SiO_3	53·29
CaO	32·86
H_2O	13·85
	<hr/> 100·00

which agree no better with our experimental numbers than the last. Neither of these formulæ, therefore, has the validity of Anderson's, which it affords me a melancholy pleasure to re-affirm on behalf of my lamented friend, under whom I did far too much work not to know what delight he took in accuracy, both of work and its representation. The water is much too high, the silica and especially the lime are too low, allowing for replacements in the latter, in Dana's formula; while the other shows too little water and too much silica.

Considering that Anderson states (*loc. cit.*) that he found

his mineral to lose water with great facility, and that the specimen I analyzed was partly opaque and had been exposed for an unknown length of time, it may seem that I should be led to the view that centrallassite is really gyrolite. This is supported by the fact of its structure being lamellar. Anderson says of gyrolite, "its crystallization in plates and not in needles, its high lustre, and the ease with which it loses water distinguish it completely from dysclasite" (okenite). To this it may be added that while this last mineral gelatinizes readily with acid, centrallassite does not; and though Anderson, curiously enough, does not allude to this important character, and the same is true of myself on the occasion above cited, I now find that the mineral I then described as gyrolite does not gelatinize, but resembles centrallassite in giving slimy silica. Notwithstanding all this, I am still held to claim individuality for centrallassite, because I proved in my paper on the three minerals, at least to my own satisfaction, that my mineral does not lose water readily. I showed that on taking portions from the interior of the mass, some "without lustre, chalk-white, and in which a radiated structure was sometimes, but not invariably, quite obvious," in two cases the percentage of water was found to be 12.29 and 12.25, which is a little above the average, 11.41, and was supposed to be accounted for by absorption of water by the white powdery form, because, as I find from my notes, a direct experiment was made with regard to the loss and absorption of water. Some fragments, selected as being nearly pure centrallassite, were powdered and weighed at once, being placed over H_2SO_4 ; the loss in forty hours was at an end and amounted to 0.53 per cent.; and on exposure to the air for two days the gain by the dried powder was complete and equalled 0.73 per cent.; on redrying over H_2SO_4 , the loss on the original weight was the same as before, and on reexposure for two hours only or less the gain was 0.59 per cent.

My opinion, from the whole evidence, is that, from the correspondence of okenite, gyrolite, and centrallassite in constituents, and their approximate quantitative composition, together with the occurrence of a few prismatic needles in tufts along with the pearly plates, or in close proximity; noticed by myself formerly in the case of the latter mineral ("two very small tufts of divergent silky transparent needles seen in the interior of the nodule, having the blowpipe characters of the laminae," *loc. cit.*), they are closely related indeed, but the merging of one in either of the others as a species is not admissible. Taking the formula I now substitute for my old one for centrallassite, that of Anderson, which I have shown anew to be

established by analyses, for gyrolite, and that in the books, which is amply supported by experiment, for okenite, we have, when they are written on both systems of notation, the following :—

	Old notation.	New notation.
Okenite .	$\dot{\text{Ca}} \ddot{\text{Si}}^2 + \dot{\text{H}}^2$	$\text{Ca Si}_2 \text{O}_5 \cdot 2 \text{H}_2 \text{O} = \text{CaO} \cdot 2 \text{SiO}_2 \cdot 2 \text{H}_2 \text{O}.$
Centrallassite	$\dot{\text{Ca}}^2 \ddot{\text{Si}}^3 + \dot{\text{H}}^2$	$\text{Ca}_2 \text{Si}_3 \text{O}_8 \cdot 2 \text{H}_2 \text{O} = 2 \text{CaO} \cdot 3 \text{SiO}_2 \cdot 2 \text{H}_2 \text{O}.$
Gyrolite .	$\dot{\text{Ca}}^2 \ddot{\text{Si}}^3 + \dot{\text{H}}^3$	$\text{Ca}_2 \text{Si}_3 \text{O}_8 \cdot 3 \text{H}_2 \text{O} = 2 \text{CaO} \cdot 3 \text{SiO}_2 \cdot 3 \text{H}_2 \text{O}.$

which show very simple relations. These minerals are all somewhat rare natives of trap-rocks ; and the associations of the last appear to be much more numerous than those of the others. I have shown that besides occurring with the minerals mentioned by Anderson, various well-known zeolites at Skye, it is found in this province on the Bay of Fundy associated with calcite and apophyllite, and also with Mordenite ("A new Mineral from the Trap of Nova Scotia," Journal of the Chemical Society, II. vol. ii. p. 100).

Stilbite.—It is curious that, abundant as stilbite is in this region, where it exhibits a large number of crystallized varieties, some of which are of great beauty, the Nova-Scotian mineral has not afforded a single analysis to the treatises on mineralogy. Among the most marked varieties may be specified honey-yellow sheaves at Partridge Island, radiated hemispheres at Margaretville and Hall's Harbour, groups of long colourless flattened crystals at the latter place, brownish flattened crystals attached by edges into groups, of which I have procured masses of considerable size from veins some inches thick at Morden, and large brown sheaves with contracted bases set together obliquely, forming curious specimens, at some of the noted localities. It is also found forming solid homogeneous masses of lamellar structure in thin veins. I analyzed such a specimen, milk-white, translucent in strong light, retaining considerable lustre on cleavage, from the trap on the shores of Annapolis County, near Margaretville or Port George, with these results :—

	Found.	Calculated.
Water . . .	16.52	17.20
Alumina . . .	17.28	16.50
Lime . . .	7.57	8.90
Soda . . .	2.10	
Silica . . .	57.32	57.40
	<u>100.79</u>	<u>100.00</u>

It follows, on comparison with the adjacent calculated per-

centages from the well-established formula, that in this case there is no variation from the normal composition, except what is frequent in the species, a replacement of lime by soda.

Sphærostitbite.—Among the forms of stilbite occurring here are to be observed crystalline incrustations on various minerals; calcite and apophyllite, for example, and crystals of stilbite are sometimes coated with layers of a mineral which seems altogether destitute of crystalline structure. I have collected, near Hall's Harbour, a number of specimens showing most curious concretionary structure. In several of these the external surfaces are perfectly smooth and rounded; and I should have considered them to be sphærostitbite; only, while agreeing in blowpipe characters, and seeming to pass in a suite of specimens directly from well-defined stilbite into such forms, they refused to gelatinize. But since I found that, according to Heddle, the gelatinization of sphærostitbite is in reality due to mesolite, and as I can find no appearance of this mineral in these specimens, I think they may be fairly placed in this subdivision (Dana's 'Mineralogy,' fifth edition, p. 443). I have found a more distinctly marked appearance of spheres, about the size of small shot, in a travelled specimen at Hants Port, at the mouth of the river Avon near Windsor; the little spheres are colourless and behave like those just mentioned; they show no mesolite; they fill a drusy cavity in crystalline trap, no doubt drifted from the south shore of the Bay of Fundy.

Cylindroid of various Minerals filling a Vapour-tube.—Upon one occasion I found in the trap near Morden, King's Co., a rather curious specimen, looking like a green rod running through the rock. There was a considerable length of it exposed; and I detached a few pieces, of which the longest measured several inches. They were quite solid, not round, but cylindroidal in form, about 2 inches in diameter. The green coating was probably Poonah earth; the interior contained a little of the same in patches and minute points throughout; but the great bulk of the mass was a mixture of dissimilar minerals, one of which agreed in appearance and leading characters with that variety of stilbite of which an analysis is given above, while the most abundant of the others was a fibrous mineral, dull and colourless; underlying these was apparently mordenite; there was also a red mineral in small points with diverging lines. Analysis of what seemed to be stilbite (I.) and of the other part (II.) gave these results—

	I.	II.
Water . . .	15·81	14·34
Alumina . . .	8·72	5·89
Lime . . .	13·60	11·08
Potassa . . .	3·35	4·71
Silica . . .	58·47	64·33
	<u>99·95</u>	<u>100·35</u>

which show that while there is an approach to the composition of stilbite, the other minerals are quite different in composition, and have introduced a good deal of lime and potash. I conclude that the solid is chiefly made up of stilbite and apophyllite with green earth. We have here, no doubt, an illustration of the action of water in filling cavities formed by the passage of vapours in the original condition of the rock. In a very interesting paper on Trap Rocks of the Connecticut Valley (Silliman's Journal, March 1875), analogous effects are described by Mr. G. W. Hawes, who says (p. 191), in reference to the chemical composition of the rocks and the contents of cavities, "That there was such a passage of vapours through the molten mass is evident; for the rock of some dykes contains long pipestem-like cavities which were made by the ascending vapours, and which are generally filled with calcite." I may mention that the contents of cavities mentioned by Mr. Hawes as occurring in the Connecticut-valley trap rocks, bear a close resemblance to those I have observed in those of Nova Scotia on the Bay of Fundy. In this circumstance we have additional evidence as to the fundamental similarity shown by Professor Dana to exist between "the eruptive rocks (or trap) of the Triassic-Jurassic areas of the Atlantic border, from Nova Scotia to the Carolines, all of which belong to one epoch, and are solely varieties of dolerite—rocks made up essentially of labradorite and pyroxene with more or less magnetic iron-ore in disseminated grains or crystals" (Silliman's Journal, August 1873, p. 105). With reference to the last-named constituent, I offer the following remarks.

Magnetite.—In various parts of the trap between Blorindon and Brier Island veins and pockets of magnetic iron-ore exist. The ore presents a general similarity of appearance: it is frequently compact and massive, sometimes crystallized on the surfaces when veins are only partially filled; it is generally associated with amethystine quartz-crystals (occasionally in specimens of very deep colour) or other forms of silica. On Digby Neck are several localities, at some of which the ore occurs with red hæmatite; altogether these deposits have furnished a few hundred tons for smelting. I made analyses of

two samples from veins in the North Mountain of Annapolis Co., known as Romans and Tremain ore, which resembled such compact ore as I have seen from other parts of this trap-region, with the following results :—

	Romans.	Tremain.
Quartz	5.46	4.94*
Magnesia and traces of lime	1.27	4.84
Oxygen (by deficiency)	24.94	25.19
Metallic iron	68.33	65.03
	93.27	90.22
	100.00	100.00

On calculating the metallic iron to magnetic oxide, there is per cent. for Romans 94.36, and for Tremain 89.80, which amount agrees so closely with the weight of the iron found added to the oxygen by loss in each case as to show that the minerals are nearly true magnetic oxide in the ratio of $\text{FeO} : \text{Fe}_2\text{O}_3$; the replacement by magnesia, so common in the species, is unequal.

Magnetic Hæmatite.—The trap of Digby Neck also affords micaceous iron-ore in small crystals disseminated in the rock and in cacholong, with agate and quartz, near St Mary's Bay, and at the same place, Johnson's, hæmatite in large modified crystals in numerous veins, some of which are 5 inches thick. A few miles east of this locality hæmatite is found beautifully crystallized in forms apparently showing its derivation from magnetite. At both places the crystallized mineral has the red streak of hæmatite, while a good deal, if not all, is more or less magnetic to the needle. The experiments of Hausmann, Henrici, H. Rose, and others show that the magnetism of hæmatite does not arise from a mixture of protoxide of iron, as is often asserted (for the pure peroxide can acquire magnetism in a high degree), but depends chiefly on the state of aggregation, the more crystalline and compact varieties being also the more highly magnetic (Nicol's 'Mineralogy,' p. 398).

Antimony.—In the course of analysis of trap rock from Margaretville my son and pupil, Mr. H. How, found decided traces of antimony; in what form it exists is not made out. This metal has not been certainly found, except with galena and to a very small amount, in this province; but it exists as sulphide in the adjoining province of New Brunswick, but not on the bay-shore opposite Margaretville; and it occurs in a geological formation very unlike the Triassic trap of Nova Scotia.

* With traces of titanium.

Specific Gravity of some of the Trap Rocks.—In the paper by Professor Dana before cited, the specific gravity of one variety of trap from this province, taken by myself for his purpose of comparison, is given. I may conclude with some additional examples, taken at about 60° Fahr.

		Spec. grav.
1.	Columnar trap, one weathered surface. West side of Digby Gut	2.994
2.	Crystalline trap, massive, above amygdaloid. Gulliver's Hole, Digby Neck	2.962
3.	Crystalline trap, massive, above amygdaloid, not very broken. Point Prim, Digby Gut	2.918
4.	Crystalline trap, massive, above amygdaloid, newly broken. East of Sandy Cove, Digby Neck	2.915
5.	Amygdaloid. Hall's Harbour	2.747
6.	" Peter's Point, Margaretville	2.697
7.	" Partridge Island	2.659

XVII. *Remarks upon Mr. Mallet's Strictures on the Mathematical Test applied to his Theory of Volcanic Energy by Mr. O. Fisher. By Mr. O. FISHER, Clk., F.G.S.**

MR. MALLET'S theory of volcanic energy is essentially a question of "how much?" which can only be decided by mathematical calculation. He has himself applied the method to the problem in his great paper; so that any argument against that mode of treatment of the subject lies against himself as well as against me. His general remark respecting the applicability of mathematical reasoning seems intended to reassure that class whom he describes towards the end of his paper as persons upon whom mathematical symbols exercise a sort of fascination like that of the serpent's glance upon small birds. He would bid them shake off their fear and be free; for he tells them "mathematical reasoning is an admirable and potent instrument for the discovery of truth, when the data upon which it is founded are exact, sufficient, and such as are sure to exist in nature. But all its validity depends upon these data," which we all know are usually unattainable. As a contrast to this description, I give Thomson and Tait's account of the matter.

"Until we know thoroughly the nature of matter, and the forces which produce its motions, it will be utterly impossible to submit to mathematical reasoning the exact conditions

* Communicated by the Author.

any physical question. It has been long understood, however, that an approximate solution of almost any problem in the ordinary branches of natural philosophy may be easily obtained by a species of *abstraction*, or rather of *limitation of the data*, such as enables us easily to solve the modified form of the question, while we are well assured that the circumstances so modified affect the result only in a superficial manner"*. .

This is the method I have taken; and if Mr. Mallet desires seriously to impugn my conclusions, he must either show that my formulæ are insufficient to give the full amount of heat developable under his theory, or else he must substitute in them such admissible values of the quantities involved as will support his views better than those that I have taken out of his own writings. His "lofty point of thermodynamics," from which he so complacently assails the conclusions of previous geologists, will not serve him now; for I occupy a station on the same range of heights, and one which commands his own. If he will do what I have suggested, we shall know whose house is, what he calls mine, "a house of cards."

Although Mr. Mallet disclaims attempting a refutation of my arguments, he has attacked them in those places where he evidently thinks them most assailable. Naturally, "for my own purposes" (that is, for the purpose of my argument—neither an unfair nor unreasonable act) I took his "small" (quarto) paper, which I did *not* represent to be part of his original memoir, and used it. It was from this paper, published in the 'Transactions,' that I took those "peascods" and put them into my "mathematical mill." They were Mr. Mallet's own "peascods;" and if he has any "wheat" in store which he holds back from the world, I shall be happy to put it also into the mill, and to see what sort of flour it will yield.

The quantity which I have represented in my formulæ by μ , μ' is not, as Mr. Mallet supposes, a coefficient of friction. True, the letter is commonly used for that purpose; and probably that led Mr. Mallet to suppose that it was so used here. But he might have observed that in such a case the dimensions of the equation would have been incorrect, and we should have had a length equal to an area. Nevertheless it is true that μ and μ' will depend respectively upon friction and adhesion, and increase and decrease with them. Now, if any one will take the trouble to look at Mr. Mallet's paper in this Magazine "On Rock-crushing &c.," p. 8, he will see it is stated that the coefficient of friction has been put as high as $\frac{3}{4}$ of the pressure; and "should this coefficient increase pro-

* Natural Philosophy, § 438.

portionately under the enormous pressures to which a discontinuous mass at several miles depth may be subjected, we can readily see that the transformed heat of friction produced by internal movements taking place in such materials after crushing has occurred, must be the source of a large amount of heat over and above that originally due to the crushing itself." It is evident from this passage that Mr. Mallet thought a large value for friction was favourable to his theory (as developed by Prof. Hilgard); and he afterwards generously assumes the smaller value 0.5 for the coefficient. I purposely gave him the advantage of the larger estimate. But I am quite content that Mr. Mallet should make use of any coefficient of friction which he thinks admissible.

The following passage strikes me as certainly unfair:—"Somewhat further on we find the author overthrowing, in the following sentence, the entire mathematical house of cards which he has with so much parade erected. 'If, however, as is more likely, the crust rests upon a fluid or viscous layer, the resistance to lateral motion will be much smaller; but we are not able to guess what it will be, so that we cannot *à priori* assign a value to μ .'" Why did not Mr. Mallet quote the sentence which follows and qualifies this?—"But a probable estimate [for μ] may be arrived at from the consideration that a great circle of the sphere cuts on an average at least three lines of weakness, as appears upon the inspection of a map showing the lines of volcanic action." In fact my formula is general, and can be applied whether there be a solid nucleus or a viscous substratum.

It is well known that some physicists insist upon a solid nucleus; and no doubt Mr. Mallet would have been quick to notice the omission had I not adverted to that case; but by altering the value of μ , the formula equally well applies to the case of a viscous layer beneath the crust; and I have so applied it, though Mr. Mallet has left it to be inferred that this most important case has been overlooked by me. The truth is that I have disproved the theory in this case as well as in the other, taking the coefficient of friction as low as $\frac{1}{10}$; and seeing that Mr. Mallet suggests $\frac{1}{2}$, this value must surely be small enough to satisfy him.

Again, referring to my statement that, "if the work of descent of the crust be not transformed into the heat of volcanic energy, it may be asked, What becomes of it?" Mr. Mallet objects to my reply, that part of it is transformed into heat within the nucleus,—and argues that this cannot be, because I assume that the nucleus itself is hotter than the heat of vulcanicity. I suppose Mr. Mallet will admit that part of the

work of descent of a hammer upon a piece of red-hot iron is transformed into heat within the iron, although the iron be hotter than the hammer becomes through the work upon it; and in principle the cases do not differ.

There is in Mr. Mallet's reply one argument which, if based upon true assumptions, would be of real importance. It is as follows:—"There is nothing to warrant the supposition that a crust 400 miles thick, which is the value our author [taking it from Mr. Mallet] assumes for k , would be compressed equally throughout its depth or crush simultaneously throughout its thickness; nor can it be assumed that volcanic activity is found uniformly diffused throughout the depth of such a crust, but must be supposed (as I have shown in my original paper, § 87) to be confined principally to the upper strata of the crust, where, as may easily be seen, in an elastic and flexible crust local lateral displacements may take place sufficient to produce crushing and volcanic action without any dragging of the crust as a whole over the nucleus."

Taking the latter supposition first; if the lateral displacements be confined to the upper strata, this merely makes the nucleus (for this purpose) larger. And my formula may be applied as for a solid earth, introducing such a value of k as will correspond to the depth to which, at any place, the movements are supposed to extend. But it is manifest that no greater temperature can be obtained under these circumstances. The case in which a high temperature would really be induced would be that wherein the whole pressure was sustained upon a small area only of a vertical section, so that, when the rocks gave way, the crushing should be confined to that area. But in a body like the earth, which has gone on cooling for so long, and whose crust has been so long subject to the compressions required by the theory, it is inconceivable that there should be vast, open, vertical chasms, with faces in contact only here and there, or even alternations of hard and soft strata, under such conditions as to have the supposed effect in localizing the work. It is, however, only on the supposition of some such arrangements of the rocks as these that there can be any analogy between the localization of heat in the crust and the instances adduced by Mr. Mallet in a former paper, such as those of the ordinary flint and steel, or the cutting of a file by means of a rotating disk of soft iron.

As a parting compliment to my paper, Mr. Mallet tells his readers that my concluding sentences contain a "string of improbable suppositions not containing any thing new." Mr. Scrope, who, ripe in years and honours, has so lately been removed from us, took a somewhat different view. To set up

his authority upon the main subject in opposition to Mr. Mallet's would be a begging of the whole question in dispute. But it will be admitted that what was novel to him in his favourite subject could hardly be otherwise than *new*. I received a letter from him in October last, in which, after expressing his gratification that the results of my calculations confirmed his long-held opinions, he added, "There is, however, one of the points you put forward which never struck me before, but which now appears to me most valuable, namely that the enormous amount of steam that has escaped from the interior in early times as well as down to the present, has been, and is, the cause of those *subsidences* of the crust, to which the basins of seas and oceans, and the crumplings of the terrestrial rocks are owing, far more than to any general *contraction* of the nucleus by cooling."

I would request any person who may happen to read this to correct the following error in my former paper (Phil. Mag. October 1875):—At p. 316, line 6, insert *k*; for "1½ mile", read "533 miles," and *delete* the following line.

XVIII. *On the Polarization of Electrodes in Water free from Air.* By J. A. FLEMING, B.Sc. (Lond.), F.C.S., Science Master in Cheltenham College*.

IN the 'Electrical News' for September 2, 1875, I published a short paper, previously read at the Meeting of the British Association at Bristol, entitled "The Decomposition of an Electrolyte by Magneto-electric Induction." In it experiments were described showing that when an electrolyte (*viz.* dilute sulphuric acid) flows in a very strong magnetic field, the electromotive force generated by its motion effects the electrolysis of the liquid, such electrolysis being held to be proved by the subsequent polarization of the electrodes. No actual liberation of gases was observed, since the electromotive force brought into play was insufficient to effect free decomposition. At the time it was read Professor H. A. Rowland raised certain doubts as to the correctness of the conclusions drawn; and his remarks were published, appended to my paper, in the 'Electrical News.' As these objections would, if correct, have deprived the experiments entirely of any little interest they might otherwise have had attaching to them, it was obviously necessary to examine them carefully.

Professor Rowland's remarks amounted practically to saying that he thought the electrolyte was not really decomposed, but

* Communicated by the Author.

that the polarization observed was in some way or other due to the presence of dissolved air, and that in air-free liquid the effect would not have been observed, at any rate with the electromotive force (about $\frac{1}{800}$ of a Daniell's cell) which came into play.

To meet these doubts two very definite questions have to be examined.

(i) In air-free water, can platinum electrodes be polarized by very small electromotive forces to the same degree and with the same facility as in ordinary aerated water?

(ii) Is this very feeble polarization really a decomposition of the electrolyte?

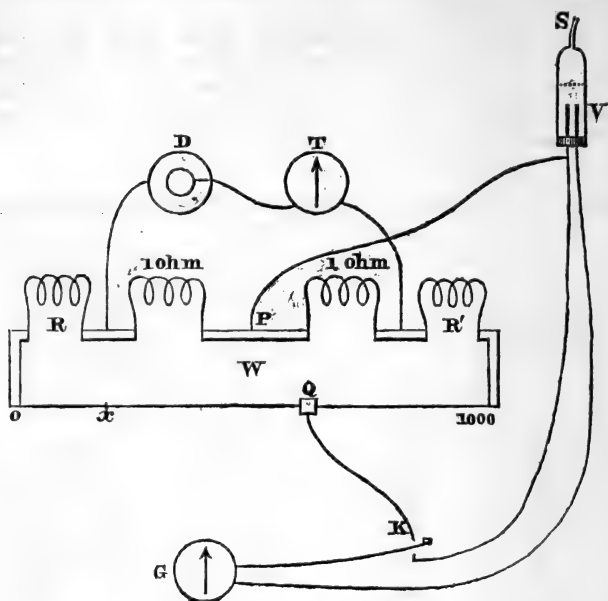
If the following facts are held to give an affirmative answer to both the above questions, the objections raised are invalid and may be dismissed.

To test the first point, it was necessary to compare the effect of a small known electromotive force acting through platinum electrodes, first in air free, next in aerated acidulated water, under identical conditions. In the former experiments the electromotive force was generated *in* the liquid by its motion; for my present purpose it was sufficient to act on it by an *external* electromotive force of measurable amount, since it is obvious the results hold good no matter how it is produced. The arrangement consisted of the following parts.

The decomposing cell or voltameter was a glass tube 30 centims. long, $2\frac{1}{4}$ centims. wide, stopped at one end by an india-rubber stopper, through which passed two platinum wires having welded to them on the interior platinum plates 8 centims. long, 2 centims. wide, placed so as to lie against the walls of the tube. The other end of the tube was connected by a narrow tube with the Sprengel pump. The platinum plates having been rendered chemically clean, the tube was three parts filled with dilute sulphuric acid, which had been previously boiled for six hours so as to expel some of the dissolved air. A vacuum was then made in the voltameter; and the apparatus remained untouched for five days, the pumping being carried on at intervals and continued during the experiments.

To obtain a small electromotive force a well-known method was made use of. A Wheatstone's bridge had its branches filled up with previously measured resistances, and the other connexions made as in the figure (p. 144). Between the points P and Q any small difference of potentials can be produced whose value is known when the distance x Q of the block from the point of equilibrium x is known. The galvanometer employed was a very sensitive mirror-galvanometer, with short needle and a long suspending fibre. The image of a fine wire across

the slit was reflected on to a scale in the usual way, and the image viewed through a telescope placed behind the galvano-



- W. Wheatstone's bridge.
- D. Daniell's cell.
- T. Tangent-galvanometer.
- V. Voltmeter.
- G. Mirror-galvanoscope.
- K. Key.
- S. Tube to Sprengel pump.

meter. By this means it was found possible to detect and read deflections of the needle amounting only to $\frac{1}{16}$ of a degree with great ease. With this delicate arrangement it was a matter of great difficulty to get the platinum plates perfectly neutral. The last traces of polarization from previous experiments subsided with extreme slowness. The following operations were then performed:—

- (i) The electrodes were made perfectly neutral.
- (ii) The galvanometer excluded from the circuit, and a direct current sent through the decomposing-cell for one minute.
- (iii) The polarization measured by the swing made by the galvanometer-needle when the electrodes were suddenly connected with it.
- (iv) The swing or excursion made by the needle when the

same direct current was started through the cell and galvanometer in one circuit, previously bringing the platinum plates to a neutral condition.

The first column in the subjoined Table gives the bridge-reading, the second the electromotive force which this is equivalent to in absolute units (C. G. S.), the third the swing of the needle with the direct current (charging), fourth the same with the polarization current (discharge).

TABLE I.—Polarization in Acidulated Water free from Air and kept *in vacuo*.

Bridge-reading.	Electromotive force.	Excursion of needle in degrees or fractions.	
		Direct current.	Polarization current.
275	12110	$\frac{1}{2}$ barely.	$\frac{1}{8}$
280	18165	$\frac{1}{2}$ full.	$\frac{1}{4}$ nearly.
285	24220	$\frac{3}{4}$ nearly.	$\frac{1}{2}$ full.
290	30275	$\frac{3}{4}$	$\frac{3}{4}$ nearly.
300	42385	1	$\frac{1}{2}$
310	54495	$1\frac{1}{2}$	$\frac{1}{2}$
320	66605	2	$\frac{1}{2}$ nearly.
330	78715	$2\frac{1}{2}$	$\frac{5}{8}$
340	90825
350	102935	3	$\frac{3}{4}$
400	115045	5	1
450	224035	7	$1\frac{3}{4}$
500	284585	10	$2\frac{1}{2}$

Since a Daniell's cell is about 100,000,000 units in electromotive force, the above Table shows that, with the galvanometer employed, the least electromotive force which, acting for one minute under these circumstances on platinum plates of 16.39 centims. surface, would leave a detectable polarization, was about $\frac{1}{8000}$ of a Daniell's cell.

The observations having been repeated several times with the same results, the dilute acid was then removed, and thoroughly aerated by shaking it up with air under pressure, and by bubbling air through it for some time. This aerated water was then returned to the voltameter, all other arrangements remaining the same, and the same observations repeated in this case as in the last. The results are appended.

TABLE II.—Polarization in Acidulated Water thoroughly well aerated.

Bridge-reading.	Electromotive force.	Excursion of needle in degrees or fractions.	
		Direct current.	Polarization current.
275	12110	$0\frac{1}{2}$ barely.	$0\frac{1}{8}$
280	18165	$0\frac{1}{2}$	$0\frac{1}{4}$
285	24220	$0\frac{1}{2}$	$0\frac{1}{4}$ barely.
290	30275	$0\frac{1}{2}$	$0\frac{1}{4}$
300	42385	$1\frac{1}{4}$	$0\frac{1}{2}$ barely.
310	54495	$1\frac{1}{2}$	$0\frac{1}{2}$
320	66605	2 barely.	$0\frac{1}{2}$
330	78715	2	$0\frac{3}{4}$ barely.
340	90825
350	102935	3	$0\frac{3}{4}$
400	115045	5	1
450	224035	7	$1\frac{1}{2}$
500	284585	10	$2\frac{3}{4}$

An examination of these two Tables shows that there is very little difference between them which cannot properly be ascribed to errors in observation. There is *no* difference in the minimum electromotive force which could give in each case a visible polarization with the galvanometer employed. But now there was one very striking difference between the two sets of experiments, not shown in the Tables, viz. the time required after each experiment for the polarization to subside. In the case of the aerated water, the polarization disappears *very much* more quickly when the plates are short, circuited than in the case of the air-free water. With this latter it was most troublesome to get rid of the last traces, even with the smallest polarizing currents. This has been previously noticed and explained by Helmholtz*. It is, however, a point not involved in our present considerations.

A consideration of these observations leads apparently to the conclusion that the presence of air in the water has no effect in creating or assisting the polarization of the electrodes under a feeble electromotive force (it is difficult to see any reason why it should), although it has a great effect in removing it when once formed. The process called by Helmholtz *electrolytic convection*, and in which dissolved air is essential, is the consequence and not the cause of polarization.

* Phil. Mag. [IV.] vol. xlvii. p. 152, Dr. Helmholtz "On Galvanic Polarization in Liquids free from Gas."

It is not true that it requires any definite electromotive force to begin to polarize electrodes in air-free water; any electromotive force, however small, will do it. Its detection is only a question of a sufficiently delicate galvanometer; and we may therefore say that any electromotive force, however feeble, acting on platinum electrodes, builds up a corresponding amount of polarization equally in air-free as in aerated water. But no liberation of free gas is possible under these circumstances, until the external electromotive force just exceeds the counter-electromotive force of the electrodes when polarized to their fullest extent.

With regard to the second question, whether polarization, however small, means literal chemical decomposition, I am not aware that it has ever been called into question. There is abundant evidence to show that it is not a mere physical state of the chemically unaltered electrolyte, but that there is *something* deposited on the electrodes which can only be the products, primary or secondary, of electrolysis. Although a voltmeter charged by a weak electromotive force resembles very closely a condenser, yet there is something very different between the state of strain or electrical displacement in a condenser's dielectric and the electrolyte after having had a current passed through it.

That even very feeble polarization does really in every case consist in the deposition of products of an unseen electrolysis in or on the electrodes receives support, in addition to many other facts, from the following:—Platinum plates were placed in the inside of a vertical glass tube, and made to communicate with the outside by wires sealed through the glass. The tube was so arranged that dilute acid could be made to flow down through the tube from a reservoir, without the plates being ever exposed to air. The tube being full of acid, the platinum plates were now polarized feebly. If the acid was allowed to flow out *slowly*, so as to gradually replace the liquid which was between the plates when polarized by other fresh acid, the plates gave, on discharge, a nearly equal current to that which they would have given if no replacement had been made; but if the liquid was made to rush down *violently* and then stopped, little or no polarization was found, provided the plates had been only slightly polarized. If the plates were defended by strips of cloth laid over them, no such removal was effected by the liquid rushing down. This all seems to indicate that there is something on the plates which can be *wiped off* mechanically. This can only be the products of electrolysis. This experiment has no analogy in a condenser. These and many other facts seem to point out that polarization is in all cases only unseen electrolysis.

In conclusion I cannot but think that the objections raised to the former experiments were groundless, and that, when it is found that a stream of acidulated water flowing down between the poles of a magnet polarizes platinum plates placed in it, we are entitled to call it chemical decomposition, since it would only be necessary to exalt the magnet's power and increase the velocity of flow in order to see, under certain precautions, the liberated gases streaming from the plates.

XIX. *Notices respecting New Books.*

An Elementary Treatise on Laplace's Functions, Lamé's Functions, and Bessel's Functions. By I. TODHUNTER, M.A., F.R.S., Honorary Fellow of St. John's College, Cambridge. Crown 8vo, pp. 348. London: Macmillan and Co. 1875.

THIS volume is designed as a continuation of the authors's two previously published volumes on the Differential and Integral Calculus respectively. Nearly two thirds of it (and these are the parts to which our remarks will be restricted) are devoted to an investigation of the properties of Laplace's Functions. This subject has been hitherto treated, at all events in English books, in subordination to their immediate application to physical questions. Thus, they have been treated by Pratt and O'Brien with a view to their application to Attractions and the Figure of the Earth; by Murphy, to the Theory of Electricity; and in the treatise on Natural Philosophy by Sir William Thomson and Professor Tait their properties are discussed, under the name of Spherical Harmonics, in an appendix to the Kinematical Introduction to that work. Mr. Todhunter has made the properties of these functions the subject of a substantial treatise, and has thereby done good service to the comparatively few students who are likely to make acquaintance with a calculus "the most singular in its nature and the most powerful in its applications that has ever appeared"*; but whose fundamental points are hard to understand, and whose applications are not without obscurity.

If the distances of two points from the origin of coordinates are r and r' , and if they contain an angle β , the distance of the points from each other is proportional to $(1 - 2a \cos \beta + a^2)^{\frac{1}{2}}$, where a denotes the ratio of r to r' . We may write this in rather a generalized form $(1 - 2ax + a^2)^{\frac{1}{2}}$. If now we denote the reciprocal of this expression by U and expand it in a series of ascending powers of a , the general term will be $P_n a^n$, where P_n or, as it is sometimes convenient to write it $P_n(x)$, is a rational and integral function of x . To these functions Mr. Todhunter gives the name of Legendre's coefficients, and he devotes 130 pages to an exposi-

* Airy, 'Figure of the Earth,' p. 192.

tion of their properties. This part of the subject does not present any serious difficulty, especially as in it we have to deal with functions of but one independent variable. It is easily shown that P_n satisfies the equation

$$\frac{d}{dx} \left\{ (1-x^2) \frac{dP_n}{dx} \right\} + n(n+1)P_n = 0: \dots\dots\dots (1)$$

this equation will have a second solution which can be obtained in a finite form, and which Mr. Todhunter calls "a coefficient of the second kind." It is easily shown that any rational function of x can be expressed in terms of Legendre's coefficients. This suggests the question, Can any function of x be so expressed? The discussion of this question yields the following answer:—If

$$f(x) = a_0 + a_1 P_1 + a_2 P_2 + \dots\dots\dots,$$

the constants a_0, a_1, a_2, \dots have determinate values. Next, no second form, such as $b_0 + b_1 P_1 + b_2 P_2 + \dots$ can exist. In the third place, the expression is always practicable, provided the series, whose general term is

$$\frac{2n+1}{2} P_n(x) \int_{-1}^1 P_n(x) f(x) dx$$

is finite; but whether the series is necessarily finite or not is not shown; and here the discussion comes to an end with this difficulty left in it. Further on, however (art. 220), the series is shown to be finite, provided x is less than unity.

It is not hard to see why these functions should be thus minutely treated. In the first place, they are important for their own sake; in the next, their discussion forms the best possible introduction to that of Laplace's functions, of which they are a sort of particular case. If we suppose the position of the points to be expressed by means of their polar coordinates (r, θ, ϕ) and (r', θ', ϕ') , the quantity denoted by x in the above expression for U becomes

$$\sin \theta \sin \theta' \cos (\phi - \phi') + \cos \theta \cos \theta',$$

and the coefficients of the expansion are no longer functions of a single variable x , but of two, viz. θ and ϕ , or it may be θ' and ϕ' , and are, in fact, Laplace's coefficients. As is well known, the n th of them (say Y_n) satisfies the equation

$$\frac{d}{d\mu} \left\{ (1-\mu^2) \frac{dY_n}{d\mu} \right\} + \frac{1}{1-\mu^2} \cdot \frac{d^2 Y_n}{d\phi^2} + n(n+1)Y_n = 0,$$

where μ stands for $\cos \theta$. The above equation (1) is strictly analogous to this equation, and is, indeed, a particular case of it. In this case we shall have numerous solutions, any one being a "Laplace's Function of the n th order," while the particular case which gives a coefficient in the expansion of U is a Laplace's Coefficient of the n th order.

From this account of the matter it will be seen how the author gradually leads the student up to the difficulties of the subject;

and particularly to what may be regarded as the main difficulty, viz. the theorem that “any function of μ and ϕ can be expressed in a series of Laplace’s functions”—a theorem of the utmost importance in mathematical physics, and about which much has been written. Mr. Todhunter gives four, and refers to a fifth method of proving it. But we infer from his statements about some, and from the way in which the other demonstrations are given, that he is not satisfied with any of them, and that he regards a proof clear of all objection as yet to seek,—an inference strengthened by a casual remark on p. 259, viz. “Admitting that the possibility of expansion in a series of Laplace’s functions has been established, we may &c.” At all events, this is what he does: he gives a method which “is in substance frequently repeated in the writings of Poisson,” and remarks that though instructive it cannot be considered perfectly conclusive; he notices M. Bonnet’s proof, and remarks that it seems unsound; he then gives two others, observing of the former that it is doubtful whether it ought to be accepted as satisfactory, and leaving the latter without remark. He then devotes two chapters to Dirichlet’s proof, the former containing introductory matter, the latter the proof itself; but he produces this proof not, apparently, because he is convinced of its cogency, but because he is “swayed by the judgment” of certain eminent mathematicians. The difficulty is an old one, and seems to have been first insisted on by Ivory; and this, it must be owned, is not a very satisfactory way of leaving it; but if there is no one decisive proof of the point at issue, no better course lies open either to student or author than the one actually adopted.

We have not space to notice the contents of the latter part of the volume; but we may just mention that it is devoted to two subjects, Lamé’s functions and Bessel’s functions. The former are a sort of generalization of Laplace’s functions; instead of the variables r, θ, ϕ , they involve λ, μ, ν , which are the *elliptic* coordinates of a point, i. e. of a point determined by the intersection of three surfaces of the second order into whose equations λ, μ, ν respectively enter as parameters. The latter belong to an entirely independent subject. If we consider the equation

$$\frac{d^2u}{dx^2} + \frac{1}{x} \cdot \frac{du}{dx} + \left(1 - \frac{n^2}{x^2}\right)u = 0,$$

and obtain u in a series of ascending powers of x , the solution is

$$Cx^n \left\{ 1 - \frac{x^2}{2(2n+2)} + \frac{x^4}{2 \cdot 4(2n+2)(2n+4)} - \&c. \right\}.$$

Now for C use the reciprocal of $2^n \Gamma(n+1)$, and this expression becomes what is called a Bessel’s function, n being any real quantity algebraically greater than -1 , and x any real quantity.

Though the work is mainly taken up with processes and with the properties of the functions under review, yet Mr. Todhunter has indicated here and there some of the applications which may be made of the formulæ. Most students will find this very useful:

they are apt to regard a book like the present as containing little more than a series of manipulations of symbols; and it is greatly to their advantage to see the results applied to concrete cases. Thus Chap. X. is devoted to the question of approximate quadrature in the case when it is effected by ordinates at unequal distances, so taken as to ensure the best result; at p. 128, there is an application of Legendre's functions to the determination of the temperature of a sphere cooling under assigned conditions; and Chap. XXV. is devoted to similar questions of a more general character. Still, questions of this kind come in only incidentally, and the student is cautioned that for applications he must look elsewhere. The author states that his demonstrations have been carefully chosen so as to bring under the attention of students some of the most instructive processes of modern analysis; and we may add that students may consider themselves fortunate in having a guide to these subjects who is not only intimately acquainted with them, but willing to expound them in a form well suited to the needs of the learner.

Solid Geometry. By PERCIVAL FROST, M.A., formerly Fellow of St. John's College, Mathematical Lecturer at King's College. Vol. I. 8vo, pp. 422. London: Macmillan and Co. 1875.

This work is the first volume of a second edition of the 'Treatise on Solid Geometry' published some years ago by Messrs. Frost and Wolstenholme. Owing to the removal of the latter gentleman from Cambridge, the preparation of the present edition has devolved on Mr. Frost, who has greatly altered the work both in form and substance, and has made additions which bring the first volume nearly up to the size of the original treatise. The subject is one that may be studied either for its own sake by those who are interested in pure Geometry, or for its use in physical investigation. The student who regards it from the latter point of view will find nearly all that he needs in the volume before us, as well as something more; thus, he might omit the chapter on quadriplanar and tetrahedral coordinates, that on the four-point coordinate system, and a few connected subjects, which occupy in all about one sixth part of the volume. In the remaining five sixths of the volume the subjects are much the same as those contained in the older books—such as were in use in Cambridge twenty or five-and-twenty years ago. Bearing in mind that the volume before us is only about half of the whole work, we obtain in some sort a measure of the enormous progress made by the science of pure Geometry within the last three or four decades.

If we compare what we may call the older part of the volume with Leroy's '*Analyse appliquée à la Géométrie des trois dimensions*'—a work to which the writers of the older Cambridge books were under great obligations—we shall find not only a great difference in the methods employed, but that incidentally much new matter has been introduced. Thus, even in so elementary a part of the subject as the chapter on planes, several points come under

notice for which we should look in vain in the earlier treatise. In the advanced parts the additions are still more numerous, as in the chapter on tortuous curves, where the subject of their curvature, their normal and osculating planes, and the connected lines and developable surfaces are admirably worked out. Here the subjects discussed are perhaps twice as numerous as those in the corresponding chapter of the earlier work. A similar remark applies to the discussion of surfaces of the second degree, which, of course, occupies a large part of the volume. Thus the reduction of the general equation of the second degree is not only effected by modern methods, but the discussion is very complete, while the chapters on confocal surfaces and on the modular and umbilical generation of "Conicoids" contain parts of the subject which have been worked out only of late years.

It will be observed that Mr. Frost uses—or, as he puts it, persists in using—the word conicoid for the locus of the equation of the second degree. We will state his reasons for doing so. After objecting to the word quadric for what seems a cogent reason, he proceeds thus:—

"The useful word 'conic' being well established, the term 'conicoid' seems to suggest all that can be required, when it is employed to designate the locus of the equation of the second degree in three dimensions, at least so long as the analogous words spheroid, ellipsoid, and hyperboloid are in use; at all events it is not open to the great objection of being equally applicable to plane curves, as is the term quadric, cubics and quartics being actually so employed in Salmon's 'Higher Plane Curves' (p. viii)."

The work is designed for the use of students of a high class, who wish to obtain a thorough acquaintance with the subject; and consequently it presupposes on their part a knowledge of the ordinary processes of analysis. Those who come to the study of it thus prepared will find few difficulties but such as are inherent in a somewhat abstruse subject. The author seems to have done all that could be desired in the way of removing the difficulties that yield to clear and brief exposition. To each chapter is added a collection of examples and exercises—between four and five hundred in all. Examples are very useful in such a work as the present; but to frame them successfully requires the exercise of mathematical knowledge and ingenuity of a high order. In the first edition these exercises were mainly due to Professor Wolstenholme, whose skill in that department of mathematics is well known. In the present edition the collection has been enlarged by contributions supplied by many Cambridge mathematicians, as well, we presume, as by the author himself.

XX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 84.]

June 17, 1875.—Joseph Dalton Hooker, C.B., President, in the Chair.

THE following Papers were read:—

“On the Change produced by Magnetization in the Electrical Resistance of Iron and Steel.—Preliminary Notice.” Communicated by Professor W. G. Adams, F.R.S.

For some time past Mr. Herbert Tomlinson, Demonstrator in the Physical Laboratory of King's College, has been engaged in carrying out a series of experiments on this subject, and also on the effect of change of tension on the electrical resistance of steel and iron wires.

In measuring the resistances of the short lengths of the wires or rods which were employed, a unit was chosen which was a small fraction of the British-Association unit.

Experiments were made with rods of soft iron about one eighth of an inch thick, with soft steel, and also with steel of different degrees of hardness.

With a rod of soft iron about 3 feet long there was an increase of resistance of about 1 per cent. on magnetizing with two Grove's cells. The whole resistance of this rod was 32 units.

The experiments were repeated with the rod placed in ice and also in water at the ordinary temperature (about 15° C.), and with nearly the same change in the resistance of the rod. The change in the temperature of the water was found to be about 1° C. during the experiment.

Another rod of soft iron was employed whose resistance was 50 units. The magnetizing current was measured by means of a tangent-galvanometer, and the resistance was measured by means of Wheatstone's bridge. There was found to be an increase in the resistance of the rod when it was converted into a magnet by sending the magnetizing current through a wire which was coiled round it in the form of a spiral.

It was found that the electrical resistance was increased when any addition was made to the strength of the magnetizing current. When the increase in the electrical resistance was divided by the square of the strength of the magnetizing current, a series of numbers was obtained which did not differ much from one another; the values of these numbers mostly lie between 3 and 4.

When the magnetizing current is considerably increased, the ratio of the increase in the resistance to the square of the magnetizing current diminishes rather rapidly.

A similar series of experiments was made with a thick knitting-needle made of *soft steel*. The resistance of the needle was 29 units. In this case also the resistance was found to *increase* when

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the strength of the magnetizing current was increased. On dividing the increase of resistance by the square of the magnetizing current, the numbers obtained from a considerable number of experiments lie between 4·7 and 5·6, showing that the ratio of the increase of resistance to the square of the magnetizing current is very nearly constant.

When the magnetizing current is considerably increased, this ratio is found to diminish, just as in the case of soft iron.

Different kinds of *hard* steel were tried.

(1) An ordinary knitting-needle, of which the resistance was 66·5 units.

On magnetizing with currents of different strengths, there was found to be a diminution in the resistance; and it was also found that the diminution of resistance increased when the strength of the current was increased. With currents varying from $\tan 15^\circ$ to $\tan 54^\circ 30'$ the diminution amounted to 4·33, *i. e.* about 6·5 per cent. of the whole resistance. The temperature *increased* about 2° C. during the experiment.

Dividing the loss of resistance by the square of the magnetizing current, the results of four sets of experiments gave the following values:—

$$\frac{.165}{(\tan 15^\circ)^2} = 2.29,$$

$$\frac{.7525}{(\tan 30^\circ)^2} = 2.26,$$

$$\frac{2.3225}{(\tan 45^\circ 30')^2} = 2.24,$$

$$\frac{4.330}{(\tan 54^\circ 30')^2} = 2.21.$$

Four Grove's cells were employed for the strongest current.

Two other experiments which had been tried previously gave results 2·30 and 2·26 for the ratio of the diminution of resistance to the square of the magnetizing current, thus showing that the diminution in the resistance is almost exactly proportional to the square of the current.

The diminution in resistance does not take place all at once, but gradually, and also ceases gradually when the current is stopped.

(2) A steel needle was also magnetized longitudinally by placing it on a copper strip at right angles to the lines of force of a current across the strip.

There was found to be diminution of resistance on increasing the current. The values obtained from two series of experiments were:—

$$\frac{.064}{(\tan 8^\circ 30')^2} = 2.88,$$

$$\frac{.192}{(\tan 15^\circ)^2} = 2.63.$$

With stronger currents this ratio was found to diminish.

On magnetizing the wires transversely by sending a current in the direction of their length, a diminution of resistance was also observed, which diminution also increased when the strength of the current was increased.

When a current was sent along the wire itself, on increasing the current there was found to be also a diminution of resistance in the case of hard steel, and an increase of resistance in the case of soft iron and soft steel.

Thus the effects produced are the same as those due to transverse magnetization by a neighbouring current.

Conclusions to be drawn from the experiments:—

(1) The effect of passing any current through a bar of hard steel is to diminish its resistance, and through a bar of soft iron or soft steel is to increase its resistance.

(2) When a bar of hard steel is magnetized by sending a current through a coil which encloses it, there is a diminution of resistance which is directly proportional to the square of the magnetizing current up to a certain limit.

(3) When soft steel or soft iron is magnetized longitudinally or transversely, there is an increase of resistance which is nearly proportional to the square of the magnetizing current.

“The Action of Light on Selenium.” By Prof. W. G. Adams, M.A., F.R.S.

The paper contains an account of several series of experiments made in December and January last on this subject with the view:—

(1) To determine whether the change in the electrical resistance of the selenium is due to radiant heat, light, or chemical action.

(2) To measure the amount of the change of resistance due to exposure to light from different sources and through various absorbing media.

(3) To determine whether the action is instantaneous or gradual; and, if possible, to measure the rate at which the action takes place.

The selenium formed one of the four resistances in a Wheatstone's bridge; and its average resistance was about $2\frac{1}{2}$ megohms.

The two resistances in the bridge, which were kept constant, were 4 and 2000; so that the resistance of the selenium was 500 times the variable resistance required to balance it.

R is taken to represent this resistance required to balance the selenium. The box containing the selenium was laid on its side and had a draw-lid, which was kept closed except when exposure was made. In front of the draw-lid was a black screen with an opening opposite to the selenium 6 centims. by $3\frac{1}{2}$ centims., in which, or in front of which, various absorbing media could be placed.

The absorbing media employed were bichromate of potash, sulphate of copper, ruby, orange, green, and blue glasses. Plates of rock-salt, alum, mica, and quartz were also employed.

With the lid of the box on, the resistance of the selenium was measured, and was found to increase slowly and regularly in consequence of the heating by the current. In most of the experiments a battery of 30 Leclanché cells was employed.

It was found that the higher the battery-power the less is the resistance of the selenium. Experiments with 5, 30, and 35 cells gave the following results:—

Resistance R with	5 cells	5400 ohms.
„	„ 35 „	4400 „
„	„ 5 „	5400 „
„	„ 30 „	4600 „

After some hours:—

Resistance R with	30 „	4800 „
„	„ 5 „	5750 „

This diminution of resistance with increased battery-power may be accounted for *in part* by leakage from the rheocord; and there may be also an opposing electromotive force similar to polarization brought into action in the selenium when the current is passing which increases with the current.

Exposure to light diminishes the resistance of selenium.

This may be accounted for by either of two hypotheses:—

(1) That light acting on the selenium sets up a polarization current in it which opposes the battery-current passing through it.

(2) That light makes the selenium a better conductor of electricity by producing a change in its surface similar to the change which it produces on the surface of a phosphorescent body, by which that body is enabled to give out light after it has been exposed.

With the same battery-power, an increase in the temperature causes an increase in the resistance of the selenium.

In December, before the extreme cold, the resistance R with 30 cells was about 5200. Throughout the extreme cold, from December 18 to January 4, the resistance R was about 4400. This was the value of R at 1 p.m. on January 1st, a bright cold day. On January 5 the temperature out of doors changed to 44° F. in the shade at 12 o'clock, and the value of R was found to be 5400. These values were obtained before the box was opened, and were the first experiments made on the days named.

When first exposed after being closed up for some days or even hours, the selenium is more sensitive to light: this sensitiveness increases with the time during which the selenium has been kept in the dark; hence the first experiment is generally not comparable with the others.

On exposure to light the resistance is diminished; but on being again eclipsed, the selenium returns in a very few minutes *nearly* to its previous resistance.

The change of resistance produced by exposure to daylight sometimes amounts to one fourth of the whole resistance of the selenium.

The experiments with various absorbing media seem to show

that the action through media which absorb all the more chemically active rays is very nearly as great as when they are not interposed, so that the chemical rays produce very little effect.

Experiments with the lime light, with rock-salt, alum, and quartz, and their combinations, two together, show that the resistance diminishes at the same rate as the illumination increases. This seems to show that the action is almost entirely due to the illuminating power of the light falling on the selenium.

Experiments with the electric light, with smoked rock-salt, alum, and a solution of iodine in bisulphide of carbon show that the obscure heat-rays do not act powerfully on the selenium.

In one series of experiments an attempt was made to separate the instantaneous effect from the gradual effect of the light.

This was done by first balancing the resistance of the selenium before exposure by a resistance R of the coils, then diminishing R by 300, 400, or 500 ohms, according to the brightness of the light, so as to get no sudden deflection when the current is made at the first instant of exposure.

It was difficult to determine beforehand by estimation what diminution of R should be made; but after several trials it was quite possible to make the sudden deflection very small, either on one side of the zero or the other, and to keep the needle near the zero by continuing to diminish the value of R as long as the exposure lasted.

In this way the effects of exposure in successive equal intervals of time can be measured.

The light allowed to pass through the coloured glasses and other absorbing media was examined by a spectroscope, and it was found that the yellowish-green rays were among the most active in altering the electrical state of the selenium.

A series of experiments was made to determine the effect of light from different sources.

A Bunsen burner was employed, and chloride of barium, chloride of strontium, thallium, and sal-ammoniac were introduced into the flame.

The effect with barium seemed to be less than with strontium.

With sal-ammoniac in the flame the effect was as great as with strontium and more lasting.

With thallium the effect was considerably greater, more gradual, as well as far more lasting than with strontium.

The effect on repeating an experiment is very much less than the effect of the first exposure with each new source of light.

Experiments were made with the Bunsen burner alone in its ordinary state and when it is rendered luminous by stopping the air-holes.

Exposure to the ordinary Bunsen flame for several seconds only caused a slight deflection of about 10 divisions of the scale. After this slight diminution of resistance the needle gradually returned to zero, and was deflected to the other side, as the heat radiated from the Bunsen burner was absorbed by the selenium.

On making the flame luminous, the needle was suddenly deflected off the scale with great rapidity.

With the 10 shunt to the galvanometer there was no deflection on exposure to the ordinary Bunsen flame; but with the luminous flame there was a sudden deflection, which increased to 250 divisions of the scale in a few seconds.

This corresponded to a change of resistance in R of about 1250 ohms.

This experiment was repeated in a slightly different way. The selenium was balanced; and before exposure to the *luminous* Bunsen flame, R was diminished by 1000 ohms. On making contact and exposing at the same instant, there was a slight deflection, showing that the sudden effect was equivalent to rather less than 1000 ohms; but in a very few seconds the needle was at rest at zero; and to keep the needle at zero the resistance was further diminished by 300 ohms.

The resistance had been diminished by one fourth of its whole amount in less than one minute in consequence of the exposure.

Exposure to an ordinary wax taper diminished the resistance of the selenium by 300,000 ohms, or about one eighth part of its whole resistance.

The illuminating powers of these sources of light were compared by means of the Bunsen photometer.

The light of the ordinary Bunsen flame could scarcely be measured, but was somewhere about $\frac{1}{200}$ part of a candle, and of the luminous Bunsen flame about 10 candles, whilst the light from the taper was at its best rather more than one fourth of a candle.

The heating effects of these three sources were compared by means of the thermo-electric pile and delicate astatic galvanometer.

At a distance of one foot from the face of the pile the deflection produced by the ordinary Bunsen flame, was $46\frac{1}{2}^\circ$ and by the luminous Bunsen flame was 52° , whilst the taper produced no effect which could be measured.

These experiments clearly show that very little effect is produced by the radiation of obscure heat, but that the effect is due almost entirely, if not entirely, to light.

As the effects produced were measured in deflections of the needle, some series of experiments were made to determine the value in resistances equivalent to the divisions of the scale, from which it appeared that with the 10 shunt to the galvanometer and with 30 cells, 20 divisions of the scale were equivalent to 100 ohms resistance; and without the shunt, 100 divisions of the scale were equivalent to from 100 to 110 ohms.

Experiments were also made to determine whether moonlight would produce any change in the electrical resistance of selenium. The experiment was made at the half-moon, when the moon was high up, so that the light fell obliquely on the window and did not shine directly on the selenium.

On throwing the moonlight on the selenium by means of a plane

mirror, the needle was at once deflected 20 divisions of the scale; on placing the mirror outside the window so as to send the moonlight perpendicularly through the window on the selenium, the deflection of the needle was 40 divisions. The window was kept closed during these experiments.

On another evening when the moon shone very obliquely on the window, and the selenium was exposed on the inside of the window directly to the moonlight, the needle was deflected 100 divisions of the scale, and the deflection increased to 150 divisions after exposure for about 3 minutes.

The change in the resistance of the selenium was from 60,000 to 70,000 ohms.

These experiments show that the action on the selenium is due principally, if not entirely, to radiations belonging to the visible part of the spectrum. Light rays of all kinds, particularly the greenish yellow, produce an instantaneous effect followed by a more or less gradual effect, which continues to increase during exposure for several minutes.

These facts suggest two hypotheses as possible explanations, which may help as guides in further experiments, but which cannot be accepted as proved without further evidence.

(1) That the light falling on the selenium causes an electromotive force in it, which opposes a battery-current passing through it, the effect being similar to the effect due to polarization in an electrolyte.

(2) That the light falling on the selenium causes a change on its surface akin to the change which it produces on the surface of a phosphorescent body, and that in consequence of this change the electric current is enabled to pass more readily over the surface of the selenium.

GEOLOGICAL SOCIETY.

November 3rd, 1875.—John Evans, Esq., V.P.R.S., President,
in the Chair.

The following communications were read:—

1. "On some new Macrurous Crustacea from the Kimmeridge Clay of the Sub-Wealden boring, Sussex, and from Boulogne-sur-Mer." By Henry Woodward, Esq., F.R.S., F.G.S.

2. "On a new Fossil Crab from the Tertiary of New Zealand." By Henry Woodward, Esq., F.R.S., F.G.S.

3. "On a remarkable Fossil Orthopterous Insect from the Coal-measures of Britain." By Henry Woodward, Esq., F.R.S., F.G.S.

4. "On the discovery of a Fossil Scorpion in the English Coal-measures." By Henry Woodward, Esq., F.R.S., F.G.S.

5. "The Drift of Devon and Cornwall, its origin, Correlation with that of the South-east of England, and place in the Glacial Series." By Thomas Belt, Esq., F.G.S.

The author described the general characters of the drift in the

district under consideration, and stated that on the uplands the drift consists of undisturbed gravels and travelled boulders, which occur only in isolated remnants on the lower ranges, and that in the lowlands and valleys within 100 feet of the present level of the sea the gravels are widely spread, and show signs of sudden and tumultuous action. Between the upland and lowland gravels he considered that great denudation had taken place. He maintained that the boulders and the materials of the gravels had been distributed by floating ice, and that their presence on the summit of Dartmoor indicated that the water on which the ice floated must have extended up to 1200 feet above the present sea-level; but he argued that this water was not that of the sea, because no old sea-beaches or remains of marine organisms are to be found in the region, although freshwater shells are preserved. He ascribed these phenomena to the presence of a great freshwater lake, produced by the drainage of Europe being dammed back by a great glacier flowing from the north-west (Greenland) down the present bed of the Atlantic, and over the northern parts of the continent. The author discussed the characters of the superficial deposits in the southern and south-eastern counties, and indicated the points in which these seemed to bear out his hypothesis.

The sequence of phenomena assumed by the author is as follows:—Accepting Mr. Tylor's notion that the actual sea-level must have been lowered during the Glacial period in consequence of the great accumulation of water in the form of ice at the poles, he seeks a point of departure for the Glacial period in the first evidence of such a lowering of the sea-level. The Weybourne sands and the marine beds of Portland Bill were deposited when the sea was at about its present level; and the Bridlington Crag probably belongs to the same period. The fossils found in these deposits show that the waters were cold. The first stage of the Glacial period is that of the older Forest-beds; and the immigration of a number of great Mammalia and of Palæolithic man, indicates that the sea had retired from the British Channel and the German Ocean, leaving these islands connected with the continent. A great river probably ran southwards through the region now submerged. The second stage is marked by the continued advance of the ice from the north, the retreat of the southern fauna and Palæolithic man, and the arrival of Arctic Mammals. The third stage saw the culmination of the Glacial period and the greatest extent of the Atlantic glacier, which reached to the coast of Europe, blocked up the English Channel, and caused the formation of an immense lake of fresh water by damming back the drainage of the whole of North-western Europe, as already indicated. In the fourth stage the Atlantic glacier began to retreat, and the sudden breaking away of the barrier of ice that blocked up the mouth of the Channel caused the tumultuous discharge of the waters of the great lake, by which the spreading of the lowland gravels was effected. To this cause the author attributes the formation of the Middle Glacial sands and gravels of Norfolk and Suffolk. During the fifth stage the ice of the German Ocean con-

tinued to retreat; but there was a temporary advance of the Atlantic glacier, which again blocked up the Channel, and produced a second great lake, which, however, did not attain so great a height as the first, and its waters were not discharged in the same tumultuous fashion. At this period the Upper Boulder-clay of Norfolk and Suffolk was formed; but the author is not convinced that this formation is represented south of the Thames, except by the "Trail" of the Rev. O. Fisher. In the sixth and last stage the Atlantic ice retreated as far as the north of Scotland: but the sea had not returned to its former level; the British Isles were connected with the continent and with each other. To this the author assigns the last great forest period, and the arrival of Neolithic man and the associated fauna from the continent.

November 17th, 1875.—John Evans, Esq., V.P.R.S., President,
in the Chair.

The following communications were read:—

1. "On a new Modification of Dinosaurian Vertebrae." By Prof. Richard Owen, C.B., F.R.S., F.G.S., &c.

2. "On the presence of the Forest-bed Series at Kessingland and Pakefield, in Suffolk, and its position beneath the Chillesford Clay." By John Gunn, Esq., M.A., F.G.S.

In this paper the author described a section from the cliff at Kessingland and Pakefield, from the examination of which he arrived at the conclusion that the Forest-bed series underlies the Chillesford Clay and Sands. At the foot of the cliff there is an estuarine deposit forming the soil of the Forest-bed, consisting of blue clay and gravel, the "Elephant-bed" of the author's former paper. Above this is the Forest-bed, containing large stools and stems of trees, but no fossil bones. This is followed by a freshwater deposit, consisting of black soil with freshwater shells, corresponding to a similar bed at Mundesley and Runton, known as the "*Unio* bed," and including the "Rootlet-bed" of oozy clay, regarded by Mr. Prestwich as an indication of the forest. The author considers the supposed rootlets to represent brushwood which succeeded the true forest. Above this come Fluvio-marine deposits, in which Crag shells occur, although but rarely. To this division the author was inclined to refer the Norwich Crag, which at Bramerton underlies the next division, regarded by the author as the Chillesford Clay and Sands. Of the overlying deposits the first is supposed to be the "Pebbly bed" by the author; it has been regarded as Middle Drift; and the uppermost is Upper Boulder-clay. The paper was illustrated by the exhibition of a fine series of bones, chiefly Cervine, from the lowest deposit noticed by the author.

December 1, 1875.—John Evans, Esq., V.P.R.S., President,
in the Chair.

The following communication was read:—

"On the Granitic, Granitoid, and associated Metamorphic Rocks

of the Lake-district." By J. Clifton Ward, Esq., F.G.S., of the Geological Survey of England and Wales*.

Part I. *On the Liquid-cavities in the Quartz-bearing Rocks of the Lake-district.*

The object of this paper was to examine into the evidence afforded by the liquid-cavities of the granitoid rocks of the Lake-district, in reference to the pressure under which these rocks may have consolidated. In the first division of the subject the geological relations of the three granitic centres of the district were considered, and it was shown that these several granitic masses probably solidified at depths varying from 14,000 to 30,000 feet. The most probable *maximum* depth for the Skiddaw granite was stated as 30,000 feet, the *maximum* for the Eskdale granite 22,000 feet, and for the Shap granite 14,000 feet. These maximum depths were arrived at by estimating the greatest thickness of strata that was ever, at one time, accumulated above the horizon of the top of the Skiddaw slates.

The mode of microscopic examination, together with a description of the precautions taken in measuring the relative sizes of the cavities and their contained vacuities, formed the second division of the paper. It was stated that all the measurements used in the calculations were made from cases in which the vacuity moved freely in the liquid of the cavity; and an approximately *perfect* case for measurement was defined to be one in which the outline of the liquid-cavity was sharply defined all round in one focus, and in which the vacuity moved freely to every part of the cavity *without going out of focus*.

Then followed the general results of the examination. Restricting the measurements to such cases as those above mentioned, the results were found to be generally consistent with one another, and with those previously obtained by Mr. Sorby in his examination of other granitic districts. From the fact that the calculated pressure in feet of rock was in all cases greatly in excess of the pressure which could have resulted from the thickness of overlying rocks, it was inferred as probable that these granitic masses were not *directly* connected with volcanic action, by which the pressure might have been relieved, but that the surplus pressure was spent in the work of elevation and contortion of the overlying rocks.

Microscopic, combined with field evidence, was thought to indicate that the Shap granite, though mainly formed at a depth similar to that at which the Eskdale granite consolidated, was yet itself finally *consolidated* at a much less depth, the mass having eaten its way upwards at a certain point, and, perhaps, representing an unsuccessful effort towards the formation of a volcanic centre.

The examination showed that the *mean* of the pressures under which the Lake-district granites probably consolidated was nearly the same as the *mean* which Mr. Sorby arrived at for those of Cornwall. In conclusion the author stated that he wished these

* Parts I. and II. were read at the Meeting of June 23.

results to be considered as preliminary only, since the *complete* investigation would necessarily occupy far more time than was at his disposal; at the same time he ventured to hope that *general* accuracy was insured, while pointing to the many little-known causes which might affect the conclusions.

Part II. *On the Eskdale and Shap Granites, with their associated Metamorphic Rocks.*

The author brought forward evidence in this paper to prove the possibility of the formation of granite by the extreme metamorphism of volcanic rocks. The passage is shown in the field, and may be observed in a complete series of hand specimens. Frequently, indeed, the actual junction is well marked; but in other cases the transition is gradual; and there occur, at some little distance from the main mass, inlying patches of what may be called bastard granite. The microscopic examination proves the passage from a distinctly fragmentary ash to a distinctly crystalline rock, and to granite itself. Also the chemical composition of the altered rocks agrees very closely with that of the granite.

Both Eskdale and Shap granite were believed to have been formed *mainly* from the rocks of the volcanic series by metamorphism at considerable depths; but the granite of Shap was thought to be in great measure intrusive amongst those particular beds which are now seen around it. A decided increase in the proportion of phosphoric acid was noted in the volcanic rocks on approaching the granite, and a decrease in carbonic acid.

Part III. *On the Skiddaw Granite and its associated Metamorphic rocks.*

The subject was treated under the three heads of (1) *Examination in the Field*, (2) *Microscopical examination*, (3) *Chemical examination*; and the following were the general results arrived at. The metamorphism of the Skiddaw slate extends for many miles around the several granitic masses, and commences by the formation of small spots which become developed into chialtolite crystals. The chialtolite *slate* passes into spotted *schist*, by the great increase of the small oblong spots arranged along planes of foliation; and mica appears. The spotted schist graduates into mica-schist, which, however, often retains to the last faint spots, and occasionally chialtolite crystals. The junction between the mica-schist and the granite is generally rather abrupt. On the whole, chemical and field evidence especially are *against* regarding the granite now exposed as the result of the extreme metamorphism of the Skiddaw slates *immediately* around it; but whether it may not have resulted from the metamorphism of underlying parts of the same series is an open question. The great contortion of the mica-schist around the granitic centres may be in part due to the at any rate *partially* intrusive character of the granite.

In an Appendix abstracts of papers by various authors who have written in connexion with the subject were given.

Part IV. *On the Quartz Felsite, Syenitic and associated Metamorphic Rocks of the Lake-district.*

This part was treated under the same three heads of *Field*, *Microscopical*, and *Chemical* evidence as the last.

The quartz felsite of St. John's Vale, and the syenitic granite of Buttermere and Ennerdale, lie for the most part at the junction of the volcanic and Skiddaw series, and seem by their line of strike, and by the occasional presence of bands of slate or volcanic rock, enclosed within or running through them, to represent the transition beds between the two series, metamorphosed in great measure *in situ*. Both microscopic and chemical evidence demonstrate the possibility of this process.

Evidence gathered in the field, and microscopic and chemical examination, all seem to suggest that the rocks of Carrock Fell &c. represent the base of the volcanic series, consisting largely of contemporaneous traps thrown into a synclinal, the axis of which ranges generally east and west, and metamorphosed into rocks of greatly varying character, such as sphærolitic felsite, hypersthene, and diorite.

Although all the various masses treated of were probably formed in the main by the metamorphism of beds *in situ*, it is probable that some parts of the resulting magma became occasionally intrusive among and absorptive of higher beds.

In an Appendix notices of papers on these rocks by other authors were given.

Part V. *General Summary.*

In this part the leading results of the four preceding divisions of this memoir were briefly brought forward, followed by the discussion of various considerations relating to metamorphism, under the following heads:—(1) Granite at various depths; (2) how far granite may be an ultimate universal product of metamorphism; (3) distribution of metamorphism (selective metamorphism); (4) classes of metamorphism.

The paper was illustrated by a large geological map of the northern part of the Lake-district, by a geological model of the Keswick district, by rock-specimens, and large coloured photographs taken from water-colour microscopic drawings made by the author.

December 15, 1875.—John Evans, Esq., F.R.S., President,
in the Chair.

The following communications were read:—

1. "Notes on the Physical Geology of East Anglia during the Glacial Period." By W. H. Penning, Esq., F.G.S.

The author wished it to be understood that his remarks were intended to form a sketch, rather than a detailed account, of the subject to which they relate. He intended to explain the origin of the so-called "middle glacial" gravels and sands, to account for their occurrence in certain areas and their non-occurrence in others where they might reasonably have been expected, also to briefly describe a certain series of gravels of doubtful age and origin in the Cambridge valley.

A short description of the geology and physical features of the district was given, and an inference drawn from the varying faunas of the "Crag" that the land was sinking during that era and until after the deposition of the Cromer "Forest-bed." Then Arctic conditions began to prevail, and the great glacial subsidence commenced; the "lower glacial" beds were formed, and succeeded by a large series of false-bedded gravels and sands, with intercalated patches of unstratified clay. These deposits run up only to a certain level, about 300 feet, never quite reaching the top of the Chalk escarpment, where the overlying Boulder-clay is invariably found resting on the older rock, without any gravel or sand between. The author inferred from this circumstance that after the deposition of the "lower" beds, and as submergence went on, the waters of the North Sea were again united to those of the Atlantic. A strong current was thus set up, which swept down from the north, bringing with it the material of which the gravels are composed, and which is found to consist of pebbles, all derived from the northern and eastern coasts, mixed with flints from the Chalk. The escarpment of this formation stood at the time above the water; but when once sufficiently submerged to admit the water over its lower portions, the conditions were altered, the current lost its force, and the deposition of gravel ceased. An occasional iceberg had dropped its load of unstratified clay, which became intercalated with the gravels; but the greater number of such bergs were quickly swept away to the south. Now the waters had access to a larger area, the formation of gravel was succeeded by that of Boulder-clay, which in the author's opinion is entirely composed of masses of clay enclosing boulders, brought down and dropped by icebergs *in mass*, which accounts for its want of stratification.

This Boulder-clay rests evenly on, and at the higher level overlaps, the "middle glacial" sands; it then caps the Chalk escarpment, and plunges down into the Cambridge valley, even to the present level of the sea; but in no instance on or beyond the escarpment does any sand or gravel intervene between it and the older geological formations, although just over the scarp (on the south side) the gravels run up to an elevation of 300 feet. The gravel-forming currents were evidently confined to the seaward side of the Chalk range, and excluded from the Cambridge valley, which is undoubtedly *præ-glacial*, and which formed at the time a large inlet, land-locked on every side but one, discharging its waters through the opening now occupied by the estuary of "The Wash."

In the Cambridge valley there are sheets of river-gravel of recent date, some patches of doubtful age, but not traceable under the Boulder-clay, and an elongated series of gravels at a level of 20 to 60 feet above the present level of the Cam. These are in some parts distant from the present course of the river, and present a striking resemblance to glacial gravels; but as they here and there contain recent shells, and taking into consideration their uniformity of level, the author concludes that they indicate an ancient course of the river Cam.

The other conclusions arrived at, after mature consideration of all the evidence hitherto obtained, are:—that a gradual passage will be found to exist from the base of the Crag up to and through the drift-deposits to those of recent date; that in East Anglia we have evidence of but one, and that a gradual, period of glacial submergence, succeeded by a corresponding movement of reelevation; and that there are no “middle glacial” deposits whatever within the area of the Cambridge valley.

2. “Denuding Agencies and Geological Deposition under the Flow of Ice and Water, with the Laws which regulate these actions, and the special bearing on River-action of observations on the Mississippi and other great rivers, and their present and past Meteorological conditions, and similar remarks on Marine Deposits, illustrated by the Irish Sea and the Chesil Beach.” By A. Tylor, Esq., F.G.S.

The writer adduced evidence by measured sections and drawings to show that the Quaternary gravels were deposited rather in a wet or pluvial than in a snowy or glacial period. He thought the denuding action of springs and the alternate action of rain and frost had been neglected. He considered Agassiz and other writers had overlooked the previous writings of Playfair, to whom he referred.

The rainfall of Westmoreland, Switzerland, and the Mississippi valley were compared in summer and winter, to prove that floods were not necessarily greater from land covered with snow than from land covered with trees and vegetation when height above the sea and local circumstances were taken into consideration. Mr. Dana’s “Great Glacier,” whose melting was to supply a Quaternary river Mississippi 50 miles wide, would require a supply equal to 625 times the present rainfall to fill it.

The melting of snow was assumed to be of such proportions by modern writers as to equal the débâcles of older geologists.

The high Swiss mountains pointed to a greater diminution of snow on high ground in the Glacial period; and he believed the clouds then discharged near the sea-level, so that the mass of snow and ice was at low levels.

It appears that in Greenland in the 80th parallel, according to Nordenskiöld, near the sea in summer there is no snow on the ground 1000 or 1500 feet above the sea. Open water at the poles must depend upon the abstraction of the vapour from the atmosphere at lower latitudes; and probably in the Glacial period the ice-cap was thickest at the 70th parallel of latitude.

Mr. Tylor thought the theories of former depressions of the land, as in the Mississippi valley, should be tested by examination for flexures. He had found (in 1868) that flexures, and not fractures, had very much affected the course of the Wealden denudation in the Quaternary period.

The laws of river-motion are very simple and precise; and as depressions and upheavals are always unequal, any great movements in the Quaternary period would affect the courses of rivers, and be traceable in their deposits.

The author had measured the *remaniés* valley gravels of Coalbrook Dale, which were associated with marine shells 200 feet above the sea, and compared their contour with ordinary valley gravels and with marine beaches, to ascertain under what probable conditions the sea had risen up the Severn valley without leaving any traces of cliffs or marine denudation except between Bridgnorth and Coalbrook Dale. The diamond gravel-deposits in Africa have a similar contour to those of Coalbrook Dale.

The position of the Moel-Tryfaen beds was first described by Trimmer in 1831. Trimmer, an excellent geologist, observed the scratches on the rocks covered by the gravel with marine remains, and noticed their ice-origin, but did not draw, unfortunately, the natural inference that there must have been a Glacial period in Wales. This great discovery or invention was left to Agassiz to propose in 1837.

The glacier-eroded lakes, much lower than Moel Tryfaen, and close to it, are free from marine remains; therefore it seems difficult to suppose a depression of 1300 feet and immersion in the sea of Tryfaen, and subsequent elevation, could have taken place without having left any marks on the land except at one spot.

The measured section of the Chesil Beach shows its close approximation to a binomial curve; and the regularity of beaches and littoral zones along the Channel teach us what are the certain consequences of land being immersed under the sea.

Mr. Tylor produced plans and sections showing how the tide actually affects the sea-bottom, and described the gorge below 50 fathoms in the Irish Sea.

He treated the tide as caused by the alternate and opposite slow movement of the deep and great mass of the Atlantic, giving motion to the water at the coast almost simultaneously as if the whole water moved as one mass over an area of thousands of square miles. The velocity of the tide of one tenth of a mile per hour in the deep sea, produced by the composition of forces a tide of a velocity of three or four miles an hour on the coast. High and low water at different ports are the direct consequences of local currents in shallow water, set in motion by the greater mass of deep water. There are points in the English Channel where within a few miles there is a difference of six hours in high water. He objected to the theory of a tidal wave travelling in one direction, and moving faster in deep water than in shallow, because the tide really travels quicker in shallow water, as his plans show.

In support of this he showed the chart of the Channel, and that the tide turned in the Irish Sea at all points, deep or shallow, almost simultaneously and synchronously with the slow tidal movement in the Atlantic. He found that in a large area of sea of 120,000 square miles, where the water averaged 67 fathoms (off the Scilly Islands) the velocity of the tide was only one mile per hour; but in the shallows near the Channel Islands, where the depth was on an average 12 fathoms, by the composition of forces the velocity of the tide increased to 6 miles an hour. If the tide were the con-

sequence of a tidal wave bringing high water, the tidal conditions of the Irish Sea would be very different from what they are described to be.

He did not find any evidence of a plane of denudation on any sea-coast, but, on the contrary, deep gorges and curved surfaces, depth varying with width, &c.

The nearest approach to a plane surface was in the estuary of the La Plata; but that flatness appeared more the consequence of deposition than denudation.

The great cuts or indentations out of coast lines where rivers discharge into the ocean, when compared with the absence of indentations in areas where there are no great rivers, but where the rocks are equally hard, showed that such denudation depended upon the alternate and opposite action of rivers and the tide. He referred to the removal of the bar of the Danube, and to the great laws which regulate the flow of water, which he illustrated by diagrams.

Hydraulics and meteorology must be studied in connexion with the lines of denudation and deposition; and however difficult and inconvenient these subjects might be, no results would be reliable unless all the physical circumstances were taken into account.

January 5, 1876.—John Evans, Esq., F.R.S., President,
in the Chair.

The following communications were read:—

1. "Historical and personal Evidences of Subsidence beneath the Sea, mainly if not entirely in the fourteenth and fifteenth centuries, of several tracts of Land which formerly constituted parts of the Isle of Jersey." By R. A. Peacock, Esq., C.E., F.G.S.

In this paper the author brings forward a great number of details, derived in part from personal observations and in part from ancient documents, to prove that a considerable submergence of land has taken place round the island of Jersey within comparatively recent times. He referred principally to the existence of a submerged forest in the Bay of St. Ouen, evidenced by the existence of stumps of trees in the sea-bottom there, and by the traditional fact that up to quite a late period fees were paid for privileges connected with the forest of St. Ouen, although the forest itself had long previously disappeared beneath the sea. From the evidence it would appear that the submergence took place at the end of the fourteenth or the beginning of the fifteenth century. The author also noticed the occurrence of peat and submarine trees in the little bay of Greve de Lecq on the north side of Jersey, and especially referred to the evidence afforded by the Ecrehous rocks and Maitre Isle, there having been in the latter a priory or chapel, supported by rents derived from the parish of Ecrehous, which is now represented only by a small islet, with the ruins of an ecclesiastical building upon it, and a range of rocks protruding but little above the sea.

2. "The Physical Conditions under which the Upper Silurian and succeeding Palæozoic Rocks were probably deposited over the Northern Hemisphere." By Henry Hicks, Esq., F.G.S.

In this paper the author, after pointing out the lines of depression explained in his former paper to the Society, now further elaborated the views then propounded by him by carrying his examination into the higher Palæozoic series and into more extensive areas. Beginning at the top of the Lower Silurian, where he first recognizes any evidence of a break in the Palæozoic rocks, he proceeded to show that this break was restricted to very limited areas, and almost entirely confined to the parts which had been first submerged, and where the greatest thickness of sediment had accumulated on both sides of the Atlantic, and hence where the præ-Cambrian crust had become thinnest. On the European side this break occurred where volcanic action had taken place, and has doubtless to be attributed to the combined action of upheaval of portions of the crust and the heaping up of volcanic material, the latter in some cases forming volcanic islets of considerable extent. He strongly objected to look upon these breaks, even in the British area, where they are most marked, as evidence of a want of continuity over other and far greater areas, or to admit that, even where there was conformity in the rocks at this point, "great intervals of time are indicated unrepresented by stratified formations." The conformity found in extensive and widely separated areas is proof also that a gradual contraction took place of an enormous portion of the crust in the northern hemisphere in Palæozoic times; and the breaks at the close of the Lower Silurian and in the Devonian are not indications of an arrest in the general subsidence. After indicating the changes which must have taken place in the climate from this gradual spreading of the water, and the evidence to be derived from the consideration of the deposits and the faunas, the author drew the following general conclusions:—

1. That the condition of the northern hemisphere at the beginning of Palæozoic time was that of immense continents in the higher latitudes, traversed by mountainous ranges of great height, but with a general inclination of the surface, on the one side (European) to the south-west and south, and on the other side (American) to the south-east and south.

2. That these continents were probably covered, at least in their higher parts, with ice and snow; and that much loose material had consequently accumulated over the plains and deeper parts, ready to be denuded off as each part became submerged. This would account for the enormous thickness of conglomerates, with boulders, grits, and sandstones, found in the early Cambrian rocks, and also to a certain extent for their barrenness in organic remains.

3. That the depression over the European and American areas was general from at least the latitude of 30° northwards; that the parts bordering the Atlantic were the first to become submerged; the lower latitudes also before the higher.

4. That the depression could not have been less altogether, for the whole of the Palæozoic, than 50,000 feet; and that conform-

able sediments to that extent are found over those parts of the areas first submerged and which remained undisturbed. That volcanic action was chiefly confined to parts of the regions which became first submerged; that the immediate cause of these outbursts was the weakness of the præ-Cambrian crust at those parts, from the great depression that had taken place, it being too thin there to resist the pressure from within, and to bear the weight of the superincumbent mass of soft sediment.

5. That the seat of volcanic action at this time was at a depth of probably not less than 25 miles, as sediments which were depressed to a depth of from 9 to 10 miles do not indicate that they had been subjected to the effect of any great amount of heat, and are free from metamorphosis.

6. That the climate at the early part of Palæozoic time was one of very considerable, if not extreme cold, and that it became gradually milder after each period of depression. That towards the close of the Palæozoic, in consequence of the elevation of very large areas, and to a great height, the climate became again more rigorous in character.

7. That the various changes which took place over the northern latitudes during Laurentian and Palæozoic times allowed marine and land life to develop and progress in those areas at interrupted periods only; consequently most of the progressive changes in the life had to take place in more equatorial areas, where the sea-bottom was less disturbed, and where the temperature was more equable. Any imperfection therefore in the Palæontological record belonging to these early times should be attributed to these and like circumstances; for wherever an approach to a complete record of any part of the chain is preserved to us, the evidence points unmistakably to an order of development, through a process of evolution from lower to higher grades of life.

XXI. Intelligence and Miscellaneous Articles.

RECOMPOSITION OF THE COMPONENT COLOURS OF WHITE LIGHT.

To the Editors of the Philosophical Magazine and Journal,

University College, Toronto,
January 13, 1876.

GENTLEMEN,

THE following experiment to illustrate the recomposition of the component colours of white light was devised by me about a year ago; and as I have looked in vain for it in the scientific periodicals and works on experimental physics with which I am acquainted, I have thought it might be useful to some of your readers.

The spectrum of the light of the sun or of the electric lamp is formed in the usual way, the slit being vertical; and the spectrum is projected on to the screen by means of a small plane mirror placed close to the prism. If this mirror be capable of vibrating through a small angle around a vertical axis, the vibrating spectrum on the screen will give rise to a horizontal band of white light with red and blue ends.

The advantages of this method over that of the vibrating prism

are so obvious that it will be surprising if it has not already occurred to some one. Not only are the results better, but the experiment is more easily performed. For the purpose I have employed the vibrating prism manufactured by Mr. Ladd of London, to one face of which I have affixed a small plane mirror.

I am, Gentlemen,

Yours faithfully,

J. LOUDON,

Prof. Math. & Nat. Phil.,

Univ. Coll. Toronto.

ON THE TRANSPARENCE OF FLAMES AND OF THE ATMOSPHERE,
AND ON THE VISIBILITY OF SCINTILLATING SIGNAL-LIGHTS.
MEMOIRS BY E. ALLARD. (ABSTRACT BY THE AUTHOR.)

I have the honour to present to the Academy several memoirs relative to some experimental and theoretic researches which I have been led to make in the exercise of my functions at the Dépôt Central des Phares.

The first memoir is concerning the transparence of flames. The burners of the lamps employed in the light-house service have diameters increasing from 3 up to 13 centims., and carry from one to six concentric wicks. By measuring the luminous intensities of the flames produced by them, it is ascertained that these intensities augment a little less rapidly than the consumption of oil; and if compared with the dimensions of the flames, the intensity per square centimetre of apparent surface is found to go on augmenting, whilst, on the contrary, the intensity per cubic centimetre of volume diminishes in proportion as the diameter becomes greater. These results can only be explained by admitting that the transparence of the flame is not absolute. I have sought to determine the coefficient of this transparence by three series of experiments:—first by measuring the intensity of different flames with flat wick, seen in front or sideways; then by means of a catadioptric reflector, which sends back towards its focus the rays which it receives, and thus obliges them to pass through the flame; and, lastly, by measuring the intensity of an electric light through a flame of large diameter. I have been led to adopt 0.80 as the mean value of the coefficient, referred to the centimetre of thickness traversed.

After establishing the theoretic formulæ giving the effective intensity of a flame as a function of its volume and its coefficient of transparency, I perceived that, to account for the observed intensities, it was necessary, with a coefficient of 0.80, to attribute to the flames mean specific intensities increasing a little with the diameter. Multiplying these specific intensities each by the volume of its flame, I find that the total quantity of light produced (or the absolute intensity) increases much more quickly than the weight of oil consumed; but, as the quantity of light absorbed by the passage of the ray through the flame increases in a still greater proportion, the difference between these two quantities (or the

effective intensity) follows a law of increase a little less rapid than the consumption of oil, as indicated by experiment.

The second memoir relates to the nocturnal transparency of the atmosphere. The observations made by the lighthouse-keepers on the visibility of the neighbouring lights consist in noting, three times each night, whether each of those lights can or cannot be perceived; so that at the end of a certain number of years it is known how many times per cent. each of the lights is visible. On the other hand, the equation of the luminous ranges, and the graphic Table which I have constructed for its ready solution in all cases, make known, for each of the lights observed, what is the lowest state of transparency at which it ceases to be seen from the place of observation. The information supplied by the keepers makes known, therefore, during what fraction n of the total duration of the nights the transparency of the air remains above a certain value represented by a coefficient a ; and as we have a series of corresponding values of n and a , we can, by constructing the points which have these values for coordinates, determine a more or less regular curve. This curve generally permits us to determine the degree of transparency above which the atmosphere is maintained during a given fraction of the year. It is found, for example, that during half the year the transparency-coefficient remains above 0.910 per kilometre on the Ocean, and above 0.932 on the Mediterranean. Similar curves constructed for different sections of the coast, and for the four seasons of the year, enable us to account for the variations undergone by the transparency of the atmosphere.

Lastly, in a third memoir, I examine the impressions produced on the visual organ by the scintillating lights obtained by the more or less rapid rotation of a system of lenses with explosions. Experiment shows that, if a luminous discharge is caused to pass before the eye, the impression experienced goes on diminishing in proportion as the velocity is augmented. If a series of discharges, succeeding one another at equal intervals, passes, with low velocities each discharge produces nearly the same effect as if it were isolated; then, when the velocity increases, the impression, while diminishing in intensity, is prolonged until the commencement of that produced by the following burst, so that one has the sensation of a light agitated with a more or less rapid trembling. If the velocity be further augmented, the trembling tends to disappear, and at last there is the sensation of a continuous light which possesses nearly the intensity of that which would be got by spreading uniformly round the horizon the quantity of light contained in the explosions.

These are the facts of which I have sought the theoretic explanation. I first admitted that, when a luminous source acting on the eye vanishes suddenly, the impression grows weaker with a velocity at each instant proportional to the value of the impression, according to the law given by Newton for cooling. From this we can easily deduce the value of the impression experienced by the eye when submitted to the action of a light variable with time; in

fact, it suffices to calculate the value, at a fixed instant, of each of the preceding impressions, and to add the results. Formulæ are thus obtained the discussion of which conducts to the same consequences as observation of the facts. I have nevertheless marked a difference: it is that the fixed light, of which we have the sensation when the velocity is very great, has in practice an intensity one or two tenths below that given by the formula. This depends, doubtless, on the circumstance that, in establishing the formula, account has been taken of all the impressions, even of those which have become too feeble isolatedly to produce a sensation, thus admitting that these impressions, though insensible by themselves, may accumulate so as to become sensible by their sum. Experience seems to indicate that it is not quite so; and if the results given by it could be sufficiently precise, there would hence, perhaps, result a means of determining in every case the time during which the impression on the retina remains sensible.—*Comptes Rendus de l'Académie des Sciences*, Dec. 6, 1875, pp. 1096–1098.

ON SOME PROPERTIES OF GALLIUM.

BY LECOQ DE BOISBAUDRAN.

After some trials which the scarcity of material rendered long and laborious, I prepared some salts of gallium sufficiently pure to give in the spectroscope, besides a magnificent spectrum of gallium, only a feeble trace of the zinc lines $\text{Zn } \alpha$ 144.62 and $\text{Zn } \gamma$ 150.05. Such a proportion of zinc is far below the limit of sensibility with the ordinary reagents.

In examining the properties of the pure salts of gallium, I have noted certain divergences from what is presented when the gallium is mixed with much zinc. In the main, this is not surprising; but it will demand fresh researches, with which I shall occupy myself as soon as I have renewed my store of gallium, which is exhausted by the following essays and what I send to-day.

Here is first the continuation of the facts observed in mixtures of gallium and zinc:—

(18)*. Ferrocyanide of potassium appears to behave with gallium as with zinc. To a dilute solution of chlorides of zinc and gallium $\frac{4}{10}$ of its volume of concentrated HCl was added, then a slight excess of yellow prussiate; lastly, to the liquor was added four times its volume of water. The whole of the gallium and all the zinc were precipitated. The ferrocyanides were washed with pretty strong HCl, and then decomposed by sulphhydrate of ammonia. The chlorhydric solution of the sulphides gave the lines of Zn and Ga brilliantly.

(19) A plate of cadmium does not precipitate any thing from a solution of chlorides of zinc and gallium, even after ebullition.

(20) With fractional precipitation by carbonate of soda, with boiling, of a ZnCl_2 containing gallium, the latter metal is concentrated in the first portions thrown down. The separation is so

* See Phil. Mag. [IV.] vol. 1. p. 414.

clean that one of the precipitates may give the lines of gallium brighter than those of zinc, while the following one shows a feeble image of the line Ga α 417 and a bright spectrum of zinc.

(21) If acetic acid be added to an ammoniacal solution of sulphates or chlorides of gallium and zinc, almost the whole of the gallium separates in the form of a gelatinous flocculent white precipitate, which is not redissolved by boiling with a considerable excess of $C_2H_4O_2$.

I now examine certain reactions of pure salts of gallium.

1. The electric spectrum of slightly concentrated chloride of gallium is very brilliant. The line 417 is much brighter than the line 404. I have not observed any other line attributable to gallium; there is certainly not any of notable intensity, at least under the physical conditions of my operations. The colour of the spark bursting on chloride of gallium is a beautiful light violet.

2. In the gas-flame I obtained only the line Ga α 417; and it was very faint and fugitive, even with a salt that gave a brilliant electric spectrum.

3. The chloride and the sulphate of gallium are precipitated by NH_3 ; but the precipitate is in great part redissolved in an excess of NH_3 . Taking up by HCl the portion not dissolved by NH_3 , and recommencing the operation, all the gallium is promptly obtained as ammoniacal solution.

4. An ammoniacal solution of sulphate or chloride of gallium is precipitated, with or without heating, by an excess of acetic acid (specimen 2). That precipitation may cease, the liquor must be extremely dilute.

5. The chloride and sulphate of gallium are not precipitated in the cold by *acid* acetate of ammonia; but the reaction takes place on heating.

6. Sulphate of gallium, evaporated and dried till the white sulphuric vapours nearly cease to be evolved, does not lose its solubility in water.

7. Sulphate of gallium is soluble in alcohol of 60 per cent.

8. I have obtained a salt which I confidently believe to be ammonio-gallic alum*. For want of a sufficient quantity, I have not been able to analyze it, nor to measure the angles; but its characters appear sufficiently well marked to convince me, reserving ulterior verification. The following facts refer to the small crystals of specimen 3.

9. Gallium-alum is soluble in cold water; but if it be heated, the salt is decomposed and the liquor becomes very thick.

10. This alum is not decomposed by hot water with the addition of acetic acid.

11. The alum readily crystallizes in cubes and octahedrons, presenting exactly the aspect of common alum; its solution, too, evaporated under the microscope, pursues the characteristic course of the known alums.

* There cannot have been present any other alkali than NH_3 and traces of Na.

12. The crystals of gallium-alum do not act on polarized light (between two Nicols at extinction).

13. A small crystal of gallium-alum was kept some time under water, and then removed into a slightly supersaturated solution of alumino-ammoniacal alum; it immediately began to grow, and determined the crystallization of the liquor.

14. With ammonia in excess, gallium-alum behaves like the other salts of this metal: a portion of the oxide is precipitated; the other part remains in solution.

15. The very acid solution of Ga_2Cl_6 is precipitated by the yellow prussiate (specimen 4).

16. The ammoniacal solution of sulphate of gallium is decomposed by the voltaic current. Metallic gallium is deposited on the platinum plate which serves as negative electrode. The positive electrode becomes covered at the same time with a whitish coating, consisting of a pellicle which is easily detached from the platinum and is insoluble in a large excess of NH_3 (specimen 5). In a first operation 1.6 milligramme was deposited in 4 hours 30 minutes on a platinum plate of about 185 square millims. superficies. The surface of the positive electrode was 877 square millims. The pile was composed of five bichromate couples (zincs 17×10 centims.) coupled in tension. The sample which I have the honour to offer to the Academy (No. 1) weighs 3.4 milligrammes: it was deposited in 5 hours 40 minutes, on a surface of 123 or 124 square millims.; the positive electrode had a surface of 877 square millims.; the current was supplied by ten bichromate elements (zincs 17×10 centims.) coupled in tension.

17. Electro-deposited gallium forms a very adhesive layer. It is hard. It cannot be very well polished by rubbing it with an agate burnisher; a better polish is obtained by forcible *compression* under the burnisher: these two operations were performed on two faces of specimen 1. By the latter method the metal acquires a brilliant lustre, and appeared to me rather whiter than platinum. When the electric current is suitably regulated, as well as the relative dimensions of the electrodes, the gallium presents a fine frosted surface of silvery whiteness, finely granulated, and strewed with brilliant points, which the microscope shows to be crystals.

18. Deposited on platinum, gallium does not oxidize notably during the washing with cold or boiling water*, nor when dried at nearly 200° in free air. It decomposes water acidulated with HCl cold, and more rapidly when heated, with brisk liberation of hydrogen.

The salts of gallium which have hitherto served for my researches were derived from the blende of Pierrefit, of which a copious supply was obligingly sent me by M. Malgor, Engineer of the Mine; but I have ascertained the presence of the new metal in other ores of zinc, especially in a transparent blende from Santander, given me by M. Friedel. I believe that gallium will be met with in all the

* The polished surface, however, is a little dulled after several days, doubtless in consequence of a slight superficial oxidation.

blendes; I hope it will not be long ere I possess more ample information on this point.

The gallium which I extracted from the blendes came really from those minerals, and not from the metallic zinc (Vieille-Montagne) employed for the precipitations; for I have obtained no traces of gallium with quantities of this zinc greater than what would have been necessary of blende to get a very pure spectral reaction of gallium.

My last investigations have confirmed the scarcity of gallium in blende. The extreme delicacy of the spectral reaction had even caused me to estimate too high the quantities obtained. I do not think I exaggerate in saying that, at the time of my first observation, I possessed at most $\frac{1}{100}$ of a milligramme of the new substance dissolved in a very little drop of liquid. I may remark that spectral examination of so minute a quantity would have been impossible before the considerable reduction which I made in the dimensions of the apparatus for obtaining electrical spectra, and without the employment (which I adopted) of very small sparks.

If, as I suppose, there is no error concerning the nature of my gallium-alum, the existence of this salt fixes the atomicity of the new element, and assigns to its oxide the same chemical function as that of alumina. The oxide of gallium will therefore be written Ga_2O_3 .—*Comptes Rendus de l'Académie des Sciences*, Dec. 6, 1875, pp. 1100–1104.

ON THE SPECTRUM OF GALLIUM. BY LECOQ DE BOISBAUDRAN.

I have again measured the wave-lengths of the lines of gallium under conditions of accuracy which the feeble brightness of the spectrum obtained did not permit me to realize at the time of my first determination*; and I have found precisely the same number for the principal line, while the less brilliant one is a little less refrangible than I at first estimated it.

With the chloride of gallium, considerably more concentrated†, which I have recently submitted to the action of the electric spark, I have observed no other lines than the two following; if, then, others should be found with highly concentrated solutions, they can only be faint.

Position on micrometer.	λ .	
α 193.72	417.0	{ Narrow, <i>strong</i> . Notably brighter in a spark of medium length than in a very short spark.
β 208.90‡	403.1	
		{ Narrow, well marked, but much fainter than α 193.72. Notably brighter with a medium spark than with a very short one.

The line α 417.0 is characteristic of gallium; this is a very delicate reaction.—*Comptes Rendus de l'Acad. des Sci.* Jan. 10, 1876, p. 168.

* *Comptes Rendus*, vol. lxxxvi. p. 494, Sept. 20, 1875; *Phil. Mag.* [IV.] vol. 1. p. 415.

† The relative intensities indicated in the description of the lines refer to the mean state of concentration of the solution now employed.

‡ The line $\text{Ga}\beta$ is much more difficult to measure than $\text{Ga}\alpha$; I nevertheless do not think that the error of λ much exceeds 0.1.

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LONDON, EDINBURGH, AND DUBLIN
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XXII. *On Crookes's Radiometer.* By G. JOHNSTONE STONEY,
M.A., F.R.S., *Secretary to the Queen's University in Ireland.*

To the Editors of the Philosophical Magazine and Journal.

Hampstead, February 19, 1876.

GENTLEMEN,

AS Mr. Crookes's Radiometer is at present deservedly attracting a large share of attention, it will probably not be without interest to many of your readers, and may perhaps dispel some illusions, to point out that the excess of pressure which he has observed is within the limits of what may be accounted for by the known properties of the material agents that are present.

I am, Gentlemen,

Yours faithfully,

G. JOHNSTONE STONEY.

1. The Radiometer, as I have seen it in Mr. Apps's shop, consists of four vanes supported at the ends of slender platinum arms, and movable in a horizontal circle on a cap resting on a steel point at the centre. I was informed that each vane consists of three folds of thin platinum foil. The faces of the vanes are presented horizontally, and are each about two square centimetres in area. One face of each vane is blackened. The whole is mounted within a small glass chamber, some six or seven centimetres across, which I was told had been exhausted by the prolonged action of a Sprengel pump.

2. When this apparatus is exposed to light falling horizon-
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tally upon it, the vanes move in the direction which indicates an excess of pressure on the blackened sides. Mr. Crookes has measured this excess of pressure; and its amount, when the light of a standard candle six inches distant was allowed to fall on blackened pith, is stated to have been 0.001772 of a grain upon a surface of two square inches (see an article on "Weighing" a Beam of Light, in 'Engineering' of the 18th February, 1876). This is a pressure of somewhat less than the hundredth of a milligramme on each square centimetre. It is the object of the present communication to show that an excess of pressure of about this amount will arise under the operation of known laws.

3. When a chamber of such a size as to hold the foregoing apparatus is exposed to the prolonged action of a good Sprengel pump, say for 18 or 24 hours, it is not likely that the outstanding pressure has been reduced lower than about the tenth of a millimetre of mercury; and as it is not likely that any practicable prolongation of the action of the pump would carry the exhaustion much further, I will assume that the pressure within the chamber is 0.1 of a millim.* This outstanding pressure is caused in part by atmospheric air, and in part by the vapours of mercury and of hydrocarbons, to which should perhaps be added vapours of platinum, glass, and other substances. But it will simplify our investigation, and not materially affect our results, to treat it as consisting entirely of atmospheric air.

4. Upon the blackened surface of the vane fell those radiations from the candle which are capable of passing through glass. These will have a heating-power not very far from that which Draper's law assigns, according to which the heat that accompanies daylight is proportional to $w_1 w_2$, w_1 and w_2 being the wave-lengths of the longest and shortest undulations for which the glass is transparent. These will heat the blackened disk in a considerable degree, but not the transparent glass. I shall assume that the disk is heated one tenth of a degree Centigrade more than the glass. The disk in turn will warm a layer of the air in contact with it. Throughout the thickness of a layer of this kind, if not interfered with,

* I cannot refrain from observing here how entirely remote such a chamber is from being empty. It follows from what we know of the number of molecules in gases at ordinary pressures, that the number remaining in this so-called vacuum will be somewhere about a unit-fourteen, *i. e.* one hundred millions of millions in every cubic millimetre (see Prof. J. Loschmidt, "Zur Grösse der Luftmoleküle," Academy of Vienna, Oct. 12, 1865; G. Johnstone Stoney on the Internal Motions of Gases, Phil. Mag., Aug. 1868; Sir William Thomson on the Size of Atoms, Nature, March 31, 1870).

the temperature varies gradually, having on one side the temperature of the surrounding air, and on the other the temperature of the disk.

5. If the chamber enclosing the apparatus contained air at atmospheric pressure and temperature, this layer would be thin. It would consist of air which has been expanded by the warmth of the disk, while the air in the rest of the chamber would by this expansion be in a trifling degree compressed. In other words, the molecules whose activity has been increased by contact with the heated disk would, in their encounters with other molecules, keep back some of them, and in this way reduce the number of molecules striking the heated disk, while this process would slightly crowd molecules into the rest of the chamber, and thus increase the number coming into collision with unheated surfaces. In this way the pressure everywhere is in a small degree raised, but everything is adjusted so that there is no excess of pressure anywhere; and this adjustment takes place in an exceedingly short period of time, so short that no sensible motion of the disk can establish itself while it is being effected. In fact the number of molecules per cubic millimetre in atmospheric air is known to be about a unit-eighteen*; the molecules are dashing about with velocities of which the average is about 500 metres per second; each meets with about 1000 millions of encounters with others in every second; and the adjustment accordingly takes place with what is promptitude as compared with visible motions (see paper on the Internal Motions of Gases, in the *Philosophical Magazine* of August 1868).

6. It is necessary for our purpose to form some estimate of the thickness of the layer of warmed air. In the absence of direct experiments†, I will assume that this layer of graduated temperature would in ordinary air be one fourth-metre thick (about as thick as a sheet of paper) if the disk were 20° Centigrade hotter than the air. This seems a very moderate estimate, judging from the copiousness of the convection currents which would quickly establish themselves if there were such a difference of temperature. And from this assumption it follows that if the temperature of the disk had been raised 0.1 of a degree before the chamber was exhausted, which I have assumed to be about the elevation of temperature that actually takes place in the radiometer, the thickness of the warmed layer of air would have been half a sixth-metre. This is a very small thickness. It is about the wave-length

* That is, a million of millions of millions.

† It will appear from the sequel that the Radiometer may be made the means of determining by experiment the thickness of this layer.

of light of mean refrangibility, and about one sixteenth of the diameter of the disks which float in human blood. Let us suppose, then, that a layer of this thickness is heated, irrespective of convection, when ordinary air is inside the apparatus.

7. On the foregoing assumptions, we can compute what the state of things will be when the chamber is exhausted. When the pressure is made to vary, it appears from the doctrine of probabilities that the value of λ (using λ to designate the length of the average excursion of a molecule, *i.e.* the distance a molecule on the average travels in the intervals between two of its encounters with other molecules) will vary inversely as $\delta^{\frac{1}{3}}$, δ being the density. Now the thickness of the layer of graduated temperature depends on λ , and will vary in the same ratio as it. We have supposed the density in our vacuum-chamber to be $\frac{1}{10,000}$ of an atmosphere; it will follow that the thickness of the heated layer in this attenuated medium would be $10,000^{\frac{1}{3}}$ times what it is in ordinary air, and would therefore become half a sixth-metre $\times 10,000^{\frac{1}{3}}$, which is more than a decimetre. It therefore reaches quite to the walls of our little vacuum-chamber; and this very materially alters the state of affairs.

8. In fact we have on one side glass at a temperature of, suppose, 15° , on the other a disk at a temperature of $15^{\circ}\cdot 1$, and between them a space which is only a part of what would be required to establish a complete gradient of temperature in the intervening air. This is equivalent to saying that some of the additional momentum communicated to molecules of air by the heated disk, instead of expending itself in interaerial collisions and thus increasing the general temperature and pressure of the air, makes its way across the intervening stratum to the opposite walls of glass, where it occasions an increased pressure against them, of which the resultant is directed perpendicularly from the disk. The momentum of the accelerated molecules which reach the glass falls after the contact of the molecules with the glass to the feebler type corresponding to its lower temperature; and it is chiefly momentum of this feebler type which makes its way to regions behind the disk. An excess of force equal and opposite to that on the glass acts against the front of the disk, and is sufficient to account for the phenomena which Mr. Crookes has investigated*. For its amount may be approximated to as follows.

* This little heat-engine may be otherwise described as follows:—There is active motion of the molecules of air in all parts of the chamber;

9. Instead of the actual condition of the molecules which come into collision with the heated disk, we may substitute one more convenient for calculation. The resulting pressure will be the same as if some moderate proportion of the molecules, say one third of them, had reached it with velocities corresponding to the temperature $15^{\circ}\cdot 1$, while the remaining two thirds reached it with velocities corresponding to 15° . We may further regard the increased pressure on the disk caused by the former class of molecules as equal in amount to the portion which is compensated by the slight reductions of density in the neighbourhood of the disk, and by the slightly increased temperature and density elsewhere, which are due to the existence of a portion of the gradient. Under this hypothesis the effect of these molecules may be left out of account. There would, however, remain the augmented pressure arising from the other two thirds of the molecules, uncompensated so far as regions behind the disk are concerned; and it is the amount of this pressure which we have now to estimate. The molecules in question reach the disk, according to the hypothesis, with velocities corresponding to 15° , and are thrown off from it with velocities corresponding to $15^{\circ}\cdot 1$. It is easy to see that the augmentation of pressure which they will produce upon the disk, will be half what would arise if they had reached the disk as well as left it with velocities corresponding to the higher temperature. This latter can be calculated by Boyle and Mariotte's law. It is two thirds of a decigramme $\times \frac{0\cdot 1}{273 + 15}$, or $0\cdot 000,023$ of a gramme per square centimetre. The uncompensated excess of pressure on the disk will, upon the assumptions we have made, be half of this, or $0\cdot 000,011,5$ of a gramme per square centimetre, the amount as determined experimentally by Mr. Crookes being $0\cdot 000,01$. Accordingly an elevation of the temperature of the blackened face of the disk to the extent of about one tenth of a degree above the temperature of the glass and the back of the disk, is enough to account for the observed pressure.

but it is more active in front of the disk than behind, and causes increased pressure there, because the addition made to the motions by the heated disk has a direction as well as a quantity; and it has a direction, because cool, *i.e.* slow-moving, molecules can crowd up to the disk and are thrown off by it with their velocities increased. This does not happen if what I have called the gradient is complete. In that case the swift molecules within the limits of the gradient are sufficiently numerous to be able to keep back the slower-moving molecules beyond; accordingly the molecules that reach the disk have as high velocities before as after their contact with it; there is therefore no resultant in any direction of their momentum. In nearly perfect gases this state of things is nearly realized; and accordingly such gases do not conduct heat.

XXIII. *On a controverted Point in Laplace's Theory of the Tides.**To the Editors of the Philosophical Magazine and Journal.*U.S. Coast Survey Office, Washington,
November 17, 1875.

GENTLEMEN,

AS I have been introduced, and very properly, by Sir William Thomson in his two recent tidal papers in the September and October Numbers of the *Philosophical Magazine* as an indorser and adopter of certain views and results of Sir G. B. Airy contained in his 'Tides and Waves,' which Sir William Thomson controverts, I beg leave to occupy a few pages of your *Magazine* on the subject.

The principal point in the *first* paper referred to is a defence of Laplace's method of determining the constant $A^{(2)}$ in his result, corresponding to K_4 in Airy's presentation of it, as quoted by Sir William Thomson in § 6—a method which has been called in question by Airy, but which in this paper is characterized as "Laplace's brilliant invention."

With regard to Laplace's extension of a general formula, which shows the relations between the constants, only following K_4 back so as to make it give the relation between K_2 and K_4 , where no such relation is found by equating the coefficients of like powers of x , thus making it determine K_4 , it is not necessary to add any thing to what has been stated by Airy on page 278 of the *Philosophical Magazine*. The only argument which Sir William Thomson advances in support of this process of Laplace's is, that it is necessary to make the meridional displacement of the water zero at the equator. His reasoning in § 7, from which he infers that Laplace's constant satisfies this condition, is based upon the final ratio between the constants, expressed by

$$K_{2k+2} = \left(1 - \frac{3}{2k}\right) K_2 k.$$

But this ratio is entirely independent of K_4 ; and whatever value K_4 may have, equation (6) gives this same relation between the constants, where k is so great that the term containing k^2 in the denominator can be neglected. By Sir William Thomson's reasoning, therefore, the condition that the meridional component of motion must vanish at the equator is satisfied with any value of K_4 whatever; and hence this condition does not determine it.

A more direct and much more satisfactory method of proceeding in this case would be to substitute the expression of a^* ,

* Misprinted u in the equation.

equation (3), in the expression of u , equation (10) in Sir William Thomson's paper; and then by putting $u=0$ we should have an equation which must be satisfied where $\cos \delta=0$, in order to satisfy the condition above. Now it is readily seen from mere inspection that equation (10) with $u=0$ is satisfied at the equator by any expression of a in which the terms contain a factor which is any power whatever of $\chi = \sin \theta$, θ being the polar distance; and hence the expression of a in equation (3) satisfies it, not only with Laplace's value of K_4 , but with any assumed value of K_4 whatever. The condition, therefore, that the meridional component of motion must vanish at the equator does not determine K_4 , and the argument in favour of Laplace's process entirely fails.

At the close of § 9 Sir William Thomson makes an important remark in italics, the truth of which must be conceded; and it therefore renders erroneous certain views entertained by Airy and adopted by myself in the case of no friction with regard to the indeterminate character of the problem; for it furnishes a condition for determining K_4 in all cases except that of the critical depths of the sea which make the period of the free tidal wave coincide exactly with that of the forced wave. The expression of a , equation (3), is composed of two parts, one depending upon and vanishing with the tidal force, and the other depending upon K_4 , which is arbitrary and cannot be determined from any condition contained within the differential equation solved, but must, as is the case with all such arbitrary constants, if determined at all, be determined by some external condition not contained in it. In order to determine K_4 , therefore, we must know what the value of a is when the force vanishes; and then, putting this value for a in equation (3), we have an equation for determining K_4 , since all the terms then in the second member of the equation depending upon the force vanish, and the others are all known functions of K_4 by reason of the relations of equation (6). Now, if we can have no semidiurnal free tidal wave except in the case of the critical depth of the sea just referred to, or if in this latter case such tides are destroyed by friction, we evidently have $a=0$ when the force vanishes; and with $a=0$ the equation can be satisfied only with $K_4=0$. In the case of the critical depth, however, where there is no friction, a becomes arbitrary, as the amplitudes of all free tidal waves are; and then K_4 becomes likewise arbitrary. Hence we have $K_4=0$ for all cases except that for the critical value of the depth. This is the value which I have maintained K_4 should have in the case of nature, in which free oscillations are destroyed by friction (Tidal Researches, § 159); but if in the

case of no friction there can be no semidiurnal tide except for the critical depth (for we are not considering any other), then we must still have $K_4=0$ in my expression of a , and instead of confining the expression with this value of K_4 to cases only when there is friction, it must be extended to all cases except that in which the sea has the critical depth referred to and is without friction, in which case K_4 is arbitrary. Sir William Thomson, therefore, while defending Laplace's process of determining this constant, has furnished us a condition for determining it, which shows that we must either have $K_4=0$ or arbitrary, and not equal to the value given by Laplace's process.

For the same reasons as in the preceding case, the similar arbitrary constant A_1 in the expression of a' which I have obtained in the solution of the equations for the diurnal tides (Tidal Researches, eq. 297), must be made zero in all cases, except that of the critical depth where there is no friction, in which case alone it becomes arbitrary. Hence my tidal expression in this case, as in the case of semidiurnal tides, with $A_1=0$, only needs to be extended so as to embrace all cases without friction, except that for the critical depth. With $A_1=0$, the expression a' does not make the diurnal tide vanish in all parts of the earth as Laplace's result does, and the expressions for the motions of the sea do not make them infinite in the case in which the rotatory motion of the earth vanishes.

It seems at first view plausible that the expression of a in the semidiurnal tide should be general, including both the free and the forced tidal waves, so that when we put the terms in the expression depending upon the tidal forces equal to zero the remaining part of the expression should represent arbitrary free oscillations; and this would be true if in satisfying the original differential equations there were not an assumption made which does not hold for the free waves. In satisfying the equations, it is assumed that they are satisfied with an oscillation which has a period which is the same as that of the forces; and this is certainly true of the forced tidal wave, but not generally true for the free tidal wave; for if we were to commence solving the original tidal equations for the case of no disturbing force of moon or sun, it would certainly be begging the question mostly to assume that they must give semidiurnal oscillations. The expression of a , therefore, with the arbitrary K_4 cannot be extended so as to embrace the free oscillations, except for the case of the critical depth where there is no friction; for where an assumption is made in the solution, the results obtained from that solution cannot be extended so as to include cases in which the assumption cannot be shown to hold.

The great improbability of some of Laplace's results obtained with his value of K_4 may be seen by comparing his tide with that of canals extending east and west around the globe. With a depth of sea of about 5.5 miles, Laplace gets, with the moon and sun in conjunction on the equator, a direct tide with a range at the equator of 36 feet. If we now suppose the whole globe to be divided into canals extending east and west around the earth, say, one or two degrees in width, so that the oscillations in the canals would be entirely independent of one another, we know that with a depth of 5.5 miles all the canals between the latitudes of about 60° north and south would have inverted instead of direct tides, with a range at the equator less than 6 feet instead of 36 feet, and those only between those parallels and the poles would have direct tides. Hence the tides in the canals are inverted over the whole globe except comparatively small areas between the parallels and the poles. If we now suppose the partitions between the canals to be removed, the conditions, of course, would be somewhat changed, and there would be north and south motions of the water also besides the principal east and west component; but still it is very improbable that the effects of these small polar seas with direct tides by means of any interchanging motion of the water between the different latitudes, would reverse the inverted tides over the whole remaining part of the globe, and cause them to be direct tides with a range at the equator of 36 feet, as given in Laplace's results.

With $K_4=0$ I have obtained in this case (Tidal Researches, § 160) a tide with a range at the equator of only about 4 feet, the tide being an inverted one, as in the case of the canals, over the greater part of the globe, and having somewhat the same range, instead of a tide completely reversed and with a range at the equator of 36 feet, as given by Laplace's value of that constant.

Again, Laplace's values of K_4 make the tide at the equator, as the depth is increased, change from an inverted to a direct tide with a depth somewhere between 1.4 and 5.5 miles, say 3.5 miles, as may be seen from Laplace's results for different depths as given by Sir William Thomson (§ 15). But in the case of the canals this change from inverted to direct tides occurs at the equator with a depth of 14 miles; and with a depth of 10.5 miles they are still inverted as far as the parallels of 30° .

With $K_4=0$ my expression of a gives for the depth of 11 miles a very small tide at the equator with a range of only 1.2 foot, the tide being still inverted as in the canals. Hence with a small increase of depth, say, with a depth of 12 miles,

$K_4=0$ would give direct instead of inverted tides at the equator. With this depth we should have in the case of the canals the tides inverted between the parallels of 22° , and all between these parallels and the poles would be direct ones. If we now suppose the partitions between the canals to be removed, it is reasonable to suppose that the predominating direct tides over the greater part of the globe, by means of the interchanging motions between the different latitudes, would reverse the tides at the equator, and hence that this reversal would occur with about this depth of the sea, as my tidal expression with $K_4=0$ gives it, and not at a depth of about 3.5 miles, as given by Laplace's values of K_4 .

Of course this comparison of the results obtained with Laplace's value of K_4 and with $K_4=0$ with those given by the canals does not amount to a demonstration; but it makes the results obtained with $K_4=0$ so probable and those obtained with Laplace's values so extremely improbable, that we should hesitate in accepting the latter without a very critical examination of all the principles and processes upon which they are based.

We now come to Sir William Thomson's *second* paper, in the October Number of the Philosophical Magazine, p. 279. In this, it is seen, he adds a constant B_0 to the expression of a' , as assumed by Airy and adopted by myself; and the expression thus becomes

$$a' = B_0 + B_2\mu^2 + B_4\mu^4 + \dots$$

This constant is properly added and supplies a deficiency in the expression as first assumed by Airy, it being one of the two implied arbitrary constants in the solution of a differential equation of the second degree, which, it will be shown, cannot in this case vanish. Retaining this constant and carrying out the solution as usual by determining the other constants by means of the equations (5) and (6) given by Sir William Thomson, we get

$$\begin{aligned} B_2 &= 0, \\ B_4 &= l(B_0 + H), \\ B_6 &= \frac{2}{3}lB_0, \\ B_8 &= \frac{1}{2}lB_0 + \frac{1}{10}l^2(B_0 + H), \\ B_{10} &= \frac{2}{5}lB_0 + \frac{4}{105}l^2B_0 + \frac{2}{25}l^2(B_0 + H), \\ &\quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \end{aligned}$$

With these values of these constants the expression of a' contains the arbitrary constant B_0 , which remains to be yet determined. But this constant cannot be determined by any condition obtained from within the differential equation itself,

as Sir William Thomson endeavours to do ; for this equation is completely satisfied by the preceding expression of a with any value of B_0 whatever ; but it must be determined by some condition outside of it ; and this condition is that the volume of the sea must be constant, or, in other words, that the mean depth of the sea must remain the same. This condition is already satisfied for the equilibrium part of this tide represented by $H(1-3\mu^2)$; and it now only remains to satisfy it with regard to the small part represented by a' . This condition, therefore, will be satisfied with the value of B_0 which satisfies the equation

$$\int a' d\sigma = 0,$$

in which σ represents the surface of the globe, and in which the integration must be extended over the whole hemisphere, that is, from $\mu=0$ to $\mu=1$.

Regarding σ as a function of μ , we have $d\sigma$ equal to $d\mu$ multiplied into a constant ; and hence, instead of the preceding equation, we have

$$\int a' d\mu = 0.$$

The integration of the first member of this equation, with the preceding expression of a' substituted, gives an expression readily deducible from that of a' by increasing the exponents and subscript members of the constants by unity, and dividing the expressions of the constant by the exponents so increased ; so that it is not necessary here to give the expression. The value of B_0 which makes this expression equal to zero is the value which satisfies the preceding condition, and the value which B_0 must have in the preceding expression of a' . With the value of B_0 thus determined, the value of μ in the preceding expression of a' which makes it zero is the cosine of the latitude of the node of the tide, so far as it is represented by a' .

The other of the two arbitrary constants referred to must be zero to satisfy the condition that the meridional component of motion must vanish at the equator, as is readily seen from an inspection of equation (2), since by equation (9) $\frac{da'}{d\mu}$ in equation (2) must vanish at the equator.

WILLIAM FERREL.

XXIV. *On the Magnetism of Steel Bars.* By Dr. CARL FROMME, *Instructor in Physics at the University of Göttingen* *.

§ 1. **T**HE present memoir was suggested by a prize-theme set by the Philosophic Faculty at Göttingen in the summer of 1873, which demanded "a closer investigation of the temporary portion of the magnetism of a steel bar with various intensities of the permanent part, and under the action of various separating forces." This problem touches a still rather obscure department of physics.

The magnetization-relations of soft iron have, in the last three decades, been so profoundly investigated by numerous observers, that, it might be thought, much more than a finishing-off of the building can hardly remain for the next period. But if we consider how long it took to attain clearness in regard to soft iron, so easy to manage in comparison with steel, it cannot be surprising that the researches hitherto carried on concerning the latter are far from having brought the desirable, or even the attainable perspicuity in the comprehension of its magnetization.

The causes of the difficulties presented by steel to the exploration of its magnetic properties are, as is well known, the following. In the first place, there is the residual magnetism which appears after the action of the magnetizing force †. Again, the individual varieties of steel behave very dissimilarly; and this gives rise to a further complication. Besides, steel can be used in the soft, in the hardened state, and hardened in different degrees; and accordingly the same sort can be subjected to examination under different conditions.

An examination of the magnetic differences resulting from the specific nature of the various kinds of steel could, in our ignorance of the molecular constitution of steel, afford us little enlightenment; and, moreover, on this point there exist a whole series of works, to add to which would appear, for the present, hardly advisable. With respect to the influence of hardening, I have, at least so far, disregarded it.

Finally, the complication resulting from the appearance of residual magnetism I have endeavoured to reduce to its least measure, by employing only such magnetizing forces as tended to preserve the polarity produced by the permanent magnetism once present.

I have therefore at present worked under the simplest condi-

* Translated from a separate impression, communicated by the Author, from Poggendorff's *Annalen*, *Ergänzungsband* vii. pp. 390-430.

† On this vide Wiedemann, *Galvanismus*, vol. ii. § 314. IV. (2nd ed.).

tions, by subjecting unhardened steel bars to magnetizing forces acting always in the same direction.

§ 2. Having regard to the reasons above touched upon, I selected only two sorts of steel, differing in specific gravity and possessing great homogeneity. Of each sort five pieces were cut; and since the theory developed by Neumann and Kirchhoff, of the magnetization of an ellipsoid of rotation, had been verified for soft iron, they were brought into the form of an elongated ellipsoid of rotation.

In Table I. are collected :—the semi- major and minor axes a and b of the ellipsoids; the constants C , determined by the equation

$$C = 4\pi\sigma(\sigma^2 - 1) \left\{ \frac{1}{2} \log \text{nat.} \frac{\sigma + 1}{\sigma - 1} - \frac{1}{\sigma} \right\},$$

$$\sigma = \frac{a^2}{a^2 - b^2};$$

the specific gravities s ; the volumes v ; and the absolute weights m ,—first, as the bars came fresh from the workshop of

TABLE I.

		$a.$		$b.$		$C.$	
		millim.	millim.	millim.	millim.		
I.	1.	40.025	40.015	12.685	12.739	1.28361	1.29081
	2.	55.355	55.305	12.505	12.500	0.82805	0.82876
	3.	70.165	70.107	12.840	12.865	0.61995	0.62239
	4.	89.990	89.863	12.645	12.650	0.42521	0.42565
	5.	100.403	100.395	10.187	10.188	0.26063	0.26069
II.	1.	40.595	40.600	12.449	12.441	1.23199	1.23081
	2.	55.423	55.412	12.599	12.592	0.83506	0.83462
	3.	70.353	70.295	12.410	12.395	0.58875	0.58842
	4.	90.245	90.270	12.628	12.597	0.42189	0.42025
	5.	99.895	99.940	10.248	10.178	0.26506	0.26221
		$s.$		$v.$		$m.$	
				cub. centim.	cub. centim.	milligr.	milligr.
I.	1.	7.82366	7.82059	26780	26756	209519	209258
	2.	7.82737	7.82123	35666	35634	279169	278700
	3.	7.81957	7.82195	48176	48106	376712	376283
	4.	7.82397	7.82274	62661	62598	490258	489689
	5.	7.82184	7.82148	47438	47374	371052	370532
II.	1.	7.83577	7.83384	25865	25817	202674	202248
	2.	7.83514	7.83221	36961	36901	289596	289019
	3.	7.83353	7.83240	47007	46786	368235	366449
	4.	7.83534	7.83366	62842	62567	492385	490125
	5.	7.83426	7.83116	47363	47144	371050	369195

the mechanician Apel, and, secondly, after being subsequently freed from their residual magnetism by incandescence.

The axes were measured by the comparator, the volumes determined by weighing in water. A comparison of the volumes so obtained with those calculated from the lengths of the axes showed a very good agreement of the corresponding values as a proof of the accurate manufacture of the ellipsoid.

As the greatest difference of the specific gravities in sort I. amounted to 0.0078 and 0.0022 respectively, and in sort II. to 0.0022 and 0.0027 respectively, the homogeneousness is so great that we may beforehand regard as possible the comparability of the results obtained with the individual bars of one sort.

§ 3. Weber's theory of magnetization, having for its basis the notion of rotatable molecular magnets, refers, as is well known, only to soft iron; in order to find its application to steel also, it needs a modification which shall take into account the phenomenon of residual magnetism.

Such a modification Maxwell* believes he has discovered in the assumption that the molecular magnets, if rotated less than a definite angle β_0 from their position of equilibrium by a magnetizing force, return to it again after the action of the force has ceased, but that if the angle of rotation β is greater than β_0 , they receive a permanent deviation amounting to an angle $\beta - \beta_0$.

No experimental confirmation of this theory has, to my knowledge, followed; probably none has been attempted. Rowland, in his paper "On Magnetic Permeability" &c. (Phil. Mag. 1873, vol. xlv. p. 155), merely says that his Tables seem to prove that, with weak magnetizing forces, nearly or quite all the magnetism of a bar is temporary.

Now we have in the two components of the earth's magnetism very weak magnetizing forces, which, if Maxwell's theory has any significance, must satisfy the requirement of producing no residual magnetism; and as for the investigation of the magnetisms excited by the terrestrial force we are in possession of a very delicate method, proposed and employed by Weber†, and first applied more extensively by Riecke‡ to the investigation of the magnetization-relations of soft iron, I have com-

* Treatise on Electricity and Magnetism, vol. ii. p. 79.

† In the sixth volume of the *Abhandl. d. Gesellsch. d. Wissensch. in Göttingen*, "Bestimmung der rechtwinkligen Componenten der erdmagnetischen Kraft in Göttingen in dem Zeitraum von 1834-1853."

‡ "Beiträge zur Kenntniss der Magnetisirung des weichen Eisens," Pogg. *Ann.* vol. cxlix.

menced my experiments by submitting according to this method the ellipsoids to the action of the two components of the terrestrial magnetic force.

In reality, between the permanent magnetisms ascertained before and after a series of observations no relation was shown which could permit any conclusion to be drawn of any alteration produced by the earth's magnetic force.

I will only sketch the method very generally; a detailed description, with development of the formulæ, will be found in the above-mentioned treatise by Riecke.

A helix is required, and connected with this a galvanometer the astatic pair of needles of which have the longest possible oscillation-period. The helix can be rotated about an axis perpendicular to its geometrical axis, and is placed with its axis of rotation in the plane of the magnetic meridian and horizontal or vertical when the vertical or horizontal component is made use of. If the helix (the surface of the coils of which should at first be perpendicular to the inducing component) is turned quickly through 180° , a current is induced in it, which expresses itself in the deflection of the needle of the galvanometer. If by the multiplication method the deflection be strengthened, the sine of half the resulting deflection-angle will be a measure for the intensity of the inducing current.

If now we fix an ellipsoid, with its principal axis coinciding with the axis of the helix, in the centre of the latter, and again rotate it 180° , to the current induced by the direct action of the terrestrial force a second will now be added, arising from the circumstance that the *magnetism of position* induced by the terrestrial-magnetic component has reversed its relative position to the ends of the helix.

Since this does not hold good of the permanent magnetism, if any, present, the latter remains perfectly inactive. In order to obtain only the current induced by the reversal of the temporary magnetic moment, it is therefore necessary to operate once with the helix alone and then with the helix and inserted ellipsoid.

Let J and i denote respectively the currents which are excited mediately by the reversal of the magnetic moment, and by direct induction, and α and β the angles of deflection of the needle of the galvanometer; then we have the relation

$$J : i = \sin \frac{\alpha}{2} - \sin \frac{\beta}{2} : \sin \frac{\beta}{2}.$$

Now

$$i = 2 \frac{e}{w} F \cdot X,$$

where ϵ is the constant of induction, w the resistance of the circuit, F the surface enclosed by the coils of the helix, X the inducing component; and

$$J = 8\pi \frac{\epsilon}{w} S \cdot M,$$

S being a constant determined by the dimensions of the helix and ellipsoid, and M the magnetic moment of the ellipsoid in the direction of its principal axis.

By inserting these values of J and i in the first equation, we get

$$M = G \cdot X,$$

$$G = \frac{\frac{1}{4\pi} \cdot \sin \frac{\alpha}{2} - \sin \frac{\beta}{2}}{\sin \frac{\beta}{2}} \cdot \frac{F}{S}.$$

Neumann's formula,

$$M = k \cdot v \cdot \frac{X}{1 + kC},$$

in which v is the volume of the ellipsoid, and C the previously mentioned constant dependent on the eccentricity, serves for the calculation of the magnetization-function k ;

$$k = \frac{G}{v + GC}.$$

In conclusion, the argument is

$$K = \frac{X}{1 + kC}.$$

§ 4. The inductor consisted of a brass tube 547 millims. long, and 43.5 in diameter, on which four layers of insulated copper wire were wound; the surface enclosed by the windings was

$$F = 1735000 \text{ squ. millims.}$$

The values of the quantity S for the five ellipsoids lie between 1.4537 and 1.4533. The amount of the components of the earth's magnetic force is taken from Kohlrausch's variation-formula*, according to which they are found to be, for the middle of February 1874,

$$V = 4.314,$$

$$H = 1.866.$$

* *Nachrichten von der Gesellsch. d. Wissensch. zu Gött.* 1868, p 159.

With these data G , and from it k and K , can be calculated as soon as the angles α and β are ascertained by observation.

On every occasion three series of observations were made for this purpose, as before and after each observation with the ellipsoid inserted the angle β , corresponding to the direct induction-current, was found by turning the helix alone. Each of the three series consisted of two sets of twelve return-points of the galvanometer-needle, half belonging to one, and half to the other extremity of the multiplication-arc. The return-points were taken successively in threes; and from the eight resulting triples the multiplication-deflection was calculated on the scale.

As an example, I give *in extenso* the notes of one observation:—

Monday, March 2, 1874, evening. Temperature 7° C.
Ellipsoid I. 5. Inducing force the horizontal component.

Helix.

<i>a.</i>	<i>b.</i>		<i>a.</i>	<i>b.</i>
784.3	740.3		785.3	741.0
784.6	740.6		785.0	740.8
784.9	740.8		785.3	741.0
784.9	740.7		785.1	741.0
785.0	740.8		785.0	741.0
785.0	741.0		785.0	741.1

<i>a-b.</i>		<i>a-b.</i>
44.15		44.15
44.2		44.4
44.25		44.05
44.1		44.0
<hr/> 44.175		<hr/> 44.15

$$n = \frac{a-b}{2} = 22.0813.$$

Helix plus Ellipsoid.

<i>b.</i>	<i>a'.</i>		<i>b.</i>	<i>a'.</i>
711.6	814.9		711.0	814.6
711.2	814.8		710.8	814.5
711.0	814.7		710.5	814.2
710.9	814.7		710.2	813.8
710.7	814.8		710.1	813.8
711.0	814.8		710.1	813.8

$a' - b'$		$a' - b'$
103·5		103·7
103·75		103·85
103·9		103·65
103·8		103·7
<hr/>		<hr/>
103·7375		103·725

$$n' = \frac{a' - b'}{2} = 51·8656.$$

Helix.

$b.$	$a.$		$b.$	$a.$
739·0	783·2		739·9	783·0
739·3	783·0		739·7	782·7
739·1	783·3		738·5	782·5
738·9	783·0		738·7	782·6
739·1	783·0		738·4	782·4
738·9	782·8		738·4	782·4

$a - b.$		$a - b.$
44·05		44·2
44·05		44·1
44·0		44·05
44·0		44·0
<hr/>		<hr/>
22·025		44·0875

$$n = \frac{a - b}{2} = 22·0281.$$

In the mean, $n = 22·0547$.

Since the distance from mirror to scale was

$$r = 3317·006 \text{ scale-divisions,}$$

the angle could be calculated by the formula

$$\sin \frac{x}{2} = \frac{1}{4r} \left(n - \frac{11}{32} \frac{n^3}{r^2} \right).$$

Hence

$$\sin \frac{\alpha}{2} = 0·00390877,$$

$$\sin \frac{\beta}{2} = 0·00166222,$$

$$\sin \frac{\alpha}{2} - \sin \frac{\beta}{2} = 0·00224655.$$

$$G = 130218,$$

$$k = 9·6467,$$

$$K = 0·53107.$$

The close accordance between the values of $a-b$ is not peculiar to this example; it appeared strikingly in almost all the observations. At the same time, however, we see how enormously small are the angles α and β , even with so delicate a galvanometer as was that made use of; at the commencement it had an oscillation-period of nearly 37 seconds (later somewhat less).

The difficulty of the observation is increased when ellipsoids of less eccentricity (as Nos. 1 and 2) are employed. Under the action of the horizontal component the difference of n and n' amounted with No. 1 to not quite five, and with No. 2 to nine scale-divisions.

During the four weeks which the observations occupied, the temperature of the apartment only varied between 2·5 and 8 degrees Centigrade, a variation which, it is self-evident, did not affect the observations.

After two pairs of values of k and K were for the first time found for each of the ellipsoids by means of the vertical components, when the horizontal components had just begun to act upon ellipsoids I. 4 and I. 5 the inductor had to undergo repair. When the injury (produced by the insulation being damaged) was repaired, the observations were continued, each of the ellipsoids being now subjected twice to the horizontal and then once more to the vertical component.

The values of G , k , and K are put together in Table II.; but the values obtained before the repair are distinguished from the rest by prefixing an asterisk (*).

§ 5. We will at present consider only the values of k and K obtained after the inductor was repaired. The following are the means:—

Vertical Component.			Horizontal Component.	
	k .	K .	k .	K .
I. 1.	15·9974	0·2003	23·5034	0·0610
2.	11·8461	0·3991	18·0285	0·1062
3.	10·4328	0·5777	12·6030	0·2121
4.	9·8564	0·8311	11·4105	0·3189
5.	8·6826	1·3222	9·6467	0·5311
II. 1.	6·1380	0·5039	11·8347	0·1199
2.	6·3318	0·6890	7·7361	0·2511
3.	5·9333	0·9601	6·4746	0·3879
4.	6·4917	1·1539	6·9191	0·4762
5.	5·8772	1·6867	5·9691	0·7228

TABLE II.

Vertical Component.					Horizontal Component.				
Ellipsoid.	Date of observation.	G.	k.	K.	Ellipsoid.	Date of observation.	G.	k.	K.
I. 1.	*Feb. 11. March 11.	20560 19894	52·8676 15·9974	0·06265 0·20034	I. 1.	Feb. 25. March 2.	20272 20091	26·7246 20·2821	0·05286 0·06904
2.	*Feb. 11. March 2. March 6.	40080 39108 39066	16·1767 11·9152 11·7770	0·29970 0·39702 0·40124	2.	Feb. 26. March 2.	40202 40516	16·9155 19·1414	0·12437 0·08798
3.	*Feb. 12. March 2. March 6.	69133 67269 67336	13·0048 10·3936 10·4720	0·47583 0·57958 0·57583	3.	Feb. 26. March 2.	68522 69232	12·0320 13·1742	0·22062 0·20358
4.	*Feb. 12. March 5.	121205 118976	10·8965 9·8564	0·76583 0·83108	4.	*Feb. 19. March 2.	117364 122182	9·2002 11·4105	0·37995 0·31893
5.	*Feb. 12. March 5.	128615 126230	9·2420 8·6826	1·26561 1·32220	5.	*Feb. 19. March 2.	124688 130219	8·3458 9·64 67	0·58778 0·53107
II. 1.	*Feb. 17. March 11. March 12.	19478 18487 18543	10·4216 6·1368 6·1393	0·31173 0·50396 0·50379	II. 1.	Feb. 26. March 1.	19691 19601	12·2567 11·4126	0·11583 0·12392
2.	*Feb. 17. March 11. March 12.	38388 37648 36738	7·8257 6·8163 5·8473	0·57256 0·64467 0·73335	2.	Feb. 26. March 1.	38665 37950	8·2729 7·1993	0·23599 0·26612
3.	*Feb. 17. March 11.	63603 63073	6·6521 5·9333	0·87750 0·96014	3.	Feb. 27. March 1.	63397 63100	6·5477 6·4014	0·38442 0·39136
4.	*Feb. 13. *Feb. 18. March 11.	110936 109107 109113	6·9167 6·4903 6·4917	1·07686 1·15408 1·15390	4.	Feb. 27. March 1.	110845 111046	6·8945 6·9437	0·47748 0·47500
5.	*Feb. 13. *Feb. 18. March 11.	109893 110178 108827	6·0267 6·0674 5·8772	1·62438 1·65407 1·68666	5.	Feb. 27. March 1.	109223 109747	5·9322 6·0060	0·72553 0·72008

It results from the two values corresponding to each ellipsoid, that k diminishes with increase of the magnetizing force, or that the latter increases faster than the magnetic moment.

The same follows if the ellipsoids of one sort are regarded as comparable, from the two series of values of sort I. obtained by the vertical and horizontal components, not so well (on account of II. 4) from the second series of values of sort II., but not from the first series of the latter.

The course of the magnetization-function k under the action of feeble forces which leave no residual magnetism behind, can therefore be represented as follows:—

Starting from a large initial value, k diminishes as the magnetizing force increases, at first quickly, then more slowly, and very soon reaches a minimum, the coming-in of which is depen-

dent on the specific nature of the steel: the harder the steel, the smaller the value of the force at which the minimum enters.

Thus, with the harder sort under the action of the vertical component the minimum appears to be reached, or even to be already passed.

Hence differently elongated ellipsoids of the same sort of steel, under a constant external magnetizing force, receive different values, which arrange themselves in a series descending as the eccentricity increases—that is, as the force acting on each particle increases.

With an increasing force, however, these deviating values converge to a common minimal boundary value.

If all the values of k obtained with sort I. be arranged in a series increasing in the arguments K , to the series with increasing K a series with regularly diminishing k corresponds, provided only that we take the two means indicated:—

k .		K .	
23.5034		0.0610	
18.0285		0.1062	
15.9974		0.2003	
12.6030		0.2121	
11.4105	} 11.6283	0.3189	} 0.3590
11.8461		0.3991	
9.6467	} 10.0398	0.5311	} 0.5544
10.4328		0.5777	
9.8564		0.8311	
8.6826		1.3222	

It hence follows that throughout its various pieces sort I. possesses the same magnetization-function (for the extent of the forces employed and for the permanent magnetism, equal to zero), since with various ellipsoids the same k appertains to an equal argument K .

Accordingly Neumann and Kirchhoff's developments have validity in the present case, as for soft iron, so also for steel.

For sort II. a series so regular cannot be constructed, probably for two reasons:—first, because this sort was less homogeneous; secondly, and chiefly, because the values of k are near the minimum, where the differences of the values and the variability of each value with increasing force are too small.

Lastly, as regards the values obtained before the repair, the first glance shows that they differ in two points from those we have just considered:—(1) The values of k obtained with the vertical component are greater than the foregoing, while the two corresponding to the horizontal component are smaller. (2) The divergences between the values for the ellipsoids of

one sort are greater: thus the difference from I. 1 to I. 5 is 43.6 against 7.3; that from II. 1 to II. 5 is 4.35; while after this such a regular divergence can no longer be shown, the values of k follow pell-mell, and will not, even in the loosest fashion, arrange themselves in a series.

If, however, on this we shall build no conclusions, since much can be urged against the comparability of the series of values on both sides, still thus much can be said—that, according to what has been above maintained, an augmentation of the vertical and a diminution of the horizontal component would have produced the deviations observed in the second series of values.

By putting the series of values obtained with I. into a system of coordinates, taking K as abscissa, k as ordinate, we get a very regular curve, whether we make use of the individual values or the means. The two curves corresponding to the vertical and the horizontal component respectively run very fairly parallel. The curves of II. proceed exactly like those of I., if we merely exclude the first two of II. 4.

§ 6. After finishing the observations with the earth-inductor, the attempt was made to test Maxwell's theory by gradually raising the magnetizing force (a constant galvanic current), commencing from a very small initial value, and thus endeavouring to ascertain the value at which, according to Maxwell, residual magnetism first makes its appearance.

The ellipsoids were fixed in the axis of a helix which, in respect of its length and diameter, satisfied the conditions of a constant magnetizing force on all points of the steel body. The helix was set up perpendicular to the magnetic meridian, to the north of a magnetometer suspended by a cocoon-thread, the magnetometer consisting of a magnetized steel ring with a mirror. The deflecting action of the magnetizing helix on the magnetometer was eliminated by a compensation-helix placed southward of it. The current-circuit consisted, further, of a tangent-compass with two windings and a mirror for reading-off (described by Kohlrausch, *Pogg. Ann.* vol. cxxxviii. in communicating a measurement with the bifilar galvanometer), and a mercury commutator for changing the current at the same time in the helices and the compass.

I moreover remark that by a suitable construction of the apparatus any influence of the current in the helices, and of the magnetized steel body, upon the needle of the tangent-compass was almost entirely avoided. When such took place with the employment of more powerful currents, it never exceeded at the utmost a deflection of the needle one scale-division, and then was always ascertained by special trials. In

the experiments now to be described a correction was never necessary.

The distance from the compass to the scale amounted to 1350·58, that of the magnetometer from the scale 1303·96 of the scale-divisions.

The magnetizing helix, in all these experiments, which had the special purpose above stated, stood at the same distance of 200·11 millims. from the magnetometer. The constant distance was necessary on account of the minute quantities to be measured, and the small distance because the deflection of the magnetometer was otherwise too insignificant.

Here, however, when I was not satisfied with comparative measurements of the residual moments expressed in scale-divisions, but wished also to make observations of the temporary moments, and to apply them to quantitative measurements for the calculation of the magnetization-function, the difficulty arose that the distances of the poles of the ellipsoids entered into the reckoning with, in part, a very great relative value to the distance 200·11 millims.

Now Riecke *, from the theoretical point of view, has developed an expression for the polar distance of uniformly magnetized bodies; and he found it particularly simple in the case of the magnet having the form of a solid of rotation.

If we assume that the fifth power of the half-length of the magnet may be neglected against the fifth power of the distance of the point affected, then the polar distance of an elongated ellipsoid of rotation whose rotation-axis a coincides with its magnetic axis, if b denotes the minor axis, is

$$L = \sqrt[3]{\frac{2}{5}(a^2 - b^2)}.$$

A trial of this formula by studying the deflections of the galvanometer-needle with different distances of the ellipsoid (which possessed a certain amount of permanent magnetism), showed a fair accordance of the magnetic moments calculated from it with the aid of the polar-distance formula. Hence Riecke's formula was afterwards always made use of without hesitation.

The ellipsoid was always inserted in the helix or taken out of it with a closed current, in order to exclude as much as possible the production of induction-currents.

Table III. (pp. 200, 201) gives the temporary moments TM , the current-intensity i , the external magnetizing force X , and (as far as it was possible to calculate them) the magnetization-function k and its argument K ; also, expressed in scale-divi-

* *Nachr. von d. Gesellsch. d. Wissensch. zu Göttingen*, 1872, No. 12; *Pogg. Ann.* vol. cxlix.

sions, the original magnetism UM, and the residual magnetism RM.

TABLE III.

	TM.	i.	X.	k.	K.	UM.	RM.
I. 1.	249660	0.7668	13.284	7.074	1.318	1.0	1.0
	294350	0.9019	15.625	7.249	1.516	1.0	1.05
	388120	1.175	20.35	8.293	1.748	1.0	1.05
	441880	1.332	23.08	8.645	1.908	1.0	1.25
	600743	1.810	31.35	8.770	2.557	1.05	1.3
	668050	2.015	34.90	8.653	2.882	1.15	1.25
I. 2.	58720	0.0995	1.7236	0.75	0.75
	88998	0.1373	2.3806	0.5	0.5
	530875	0.7931	13.74	10.53	1.414	0.5	0.7
	766030	1.129	19.56	12.13	1.770	0.5	0.8
	956700	1.420	24.60	11.24	2.386	0.7	1.0
	1035500	1.536	26.61	11.307	2.568	1.0	1.2
I. 3.	109420	0.1004	1.739	6.863	0.3309	2.1	2.0
	153300	0.1392	2.411	7.255	0.4385	1.95	2.1
	247440	0.2195	3.803	8.303	0.6186	1.95	1.9
	904100	0.789	13.67	9.219	2.036	1.85	2.5
	972900	0.8448	14.636	9.544	2.116	2.1	2.45
	1230200	1.075	18.62	9.154	2.79	2.1	2.5
I. 4.	1523250	1.322	22.897	9.595	3.296	2.5	3.1
	182767	0.0999	1.7313	5.939	0.4911	0.9	0.9
	257900	0.1352	2.342	6.956	0.5917	0.9	0.95
	432200	0.2231	3.865	7.402	0.9320	0.75	0.9
	628500	0.3141	5.441	8.527	1.180	0.75	1.0
	730750	0.3614	6.261	8.959	1.306	0.9	0.85
I. 5.	1154840	0.5719	9.969	8.889	2.073	0.9	1.1
	1293200	0.6406	11.008	8.888	2.322	0.9	1.2
	1392000	0.6932	12.01	8.666	2.563	1.05	1.3
	1594740	0.781	13.223	9.392	2.710	1.15	1.4
	1848000	0.905	15.68	9.395	3.139	1.4	1.75
	210886	0.0964	1.670	8.695	0.5113	11.05	10.85
I. 5.	288800	0.1370	2.373	7.745	0.786	11.0	10.85
	320250	0.1521	2.636	7.706	0.8762	10.85	11.0
	472256	0.2285	3.872	7.791	1.277	11.15	11.2
	668500	0.3087	5.349	8.408	1.676	11.1	11.3
	789200	0.3645	6.315	8.408	1.979	11.05	11.15
	947901	0.4457	7.722	7.949	2.514	11.35	11.23
II. 1.	1226500	0.5697	9.87	8.253	3.131	11.15	11.65
	1386560	0.6423	11.13	8.328	3.509	11.3	11.7
	133900	0.4412	7.644	4.089	1.266	0.5	0.5
	179100	0.5724	9.916	4.998	1.386	0.4	0.5
	218825	0.6942	12.03	5.277	1.603	0.25	0.45
	252470	0.7957	13.79	5.542	1.761	0.2	0.5
II. 1.	280600	0.8979	15.555	4.953	2.19	0.45	0.55
	331860	1.04	18.03	5.78	2.22	0.45	0.5
	371200	1.17	20.27	5.557	2.583	0.55	0.65
	422930	1.331	23.06	5.608	2.915	0.65	0.7
	490050	1.535	26.59	5.83	3.25	0.5	0.75

TABLE III. (*continued*).

	TM.	<i>i</i> .	X.	<i>k</i> .	K.	UM.	RM.
II. 2.	63010	0.1013	1.755	4.9	4.95
	88245	0.1423	2.466	4.8	4.85
	269400	0.4452	7.714	4.48	1.627	4.8	5.05
	363200	0.5728	9.924	5.72	1.718	4.95	5.1
	398300	0.62	10.742	6.181	1.743	5.0	5.25
	453540	0.7124	12.342	5.857	2.095	5.35	5.55
	581800	0.8988	15.57	6.488	2.426	5.25	5.45
	708100	1.094	18.95	6.494	2.95	5.35	6.2
II. 3.	96770	0.0999	1.731	2.35	2.35
	142920	0.1401	2.427	4.773	0.637	2.15	2.25
	229010	0.2204	3.818	5.128	0.934	2.1	2.2
	460300	0.4452	7.714	5.025	1.949	2.35	2.35
	601100	0.5719	9.909	5.372	2.38	2.25	3.0
	661600	0.6241	10.81	5.571	2.526	2.25	2.75
II. 4.	166360	0.0999	1.731	4.308	0.6144	2.15	2.25
	219200	0.1379	2.388	3.805	0.9166	2.15	2.2
	420900	0.2226	3.857	6.497	1.031	2.45	2.35
	673500	0.3618	6.268	6.136	1.746	2.65	2.65
	819500	0.4435	7.683	5.978	2.181	2.7	3.0
	1056300	0.5715	9.901	5.982	3.537	2.65	3.4
II. 5.	178900	0.1	1.724	5.229	0.723	0.95	0.8
	247120	0.1383	2.396	5.15	1.013	0.85	0.8
	273930	0.153	2.651	5.173	1.118	0.8	0.75
	398010	0.2226	3.857	5.158	1.63	1.3	1.0
	564000	0.3078	5.333	5.471	2.177	0.85	1.05
	647100	0.3542	6.136	5.431	2.515	1.25	1.6
	766500	0.4194	7.265	5.439	2.976	1.2	2.25
	825300	0.4448	7.706	5.642	3.161	1.8	2.25
	975900	0.5345	9.26	5.425	3.79	2.0	3.1

§ 7. Before we draw the conclusions to be deduced from this Table, we will again lay before us the alternatives which are here to be decided. There are two cases possible; either the residual magnetism converges simultaneously with the magnetizing force towards zero, or else the residual magnetism reaches zero already for a finite value of the force.

Now the Table shows that, at least with the more-elongated ellipsoids, a value of the magnetizing force can be approximately fixed, at which the increment of the magnetism already present (UM) first exceeds the errors of observation. Residual magnetism is first demonstrable for the following values of the argument K and current-intensity *i*:—

	K.	<i>i</i> .		K.	<i>i</i> .
I. 1.			II. 1.		
2.	2.3	1.3	2.	2.0	0.7
3.	2.0	0.8	3.	2.2	0.5
4.	2.3	0.6	4.	2.1	0.4
5.	2.8	0.5	5.	2.2	0.3
Mean .	2.35			2.13	

The harder the steel, the feebler is the magnetizing force at which residual magnetism enters.

This result was to be expected beforehand, according to the representation which the theory of rotatable molecular magnets gives of the process of magnetization.

The molecular forces oppose the placing of the molecules in positions of magnetic equilibrium when the molecules are acted on by magnetizing forces; but, inasmuch as they have themselves undergone a change by the rotation of the molecules, they also resist the rotation of the latter back to their unmagnetic positions, and keep them fixed in certain magnetic ones after the force has ceased to operate. Now, as the molecular forces are more intense in the harder sort of steel than in the softer, the temporary magnetization turns out less in the former; and, on the contrary, the residual magnetism is greater, because the molecular forces are more altered on the rotation of the molecules.

That with the more-elongated ellipsoids residual magnetism commences earlier than with the less-elongated ones receives its explanation from this—that in the former, with an equal *external* magnetizing force, the sum of the forces acting on each molecular magnet is greater. The greater the longitudinal dimension of a body in comparison with its transverse dimension, the more of that part of the reciprocal action of the molecular magnets which is opposed to the external magnetizing force vanishes, while at the same time that which supports it and acts in the same direction is increased.

On the other hand, between the values of the argument K no definite relation appears to subsist. We may infer that residual magnetism enters at the same value of the argument K of the magnetization-function in differently elongated ellipsoids, at least of the *same* sort of steel. Only I. 5 shows a considerably greater value of the argument; on this see § 9.

Since, however, in such investigations a limit is always reached at which a normal variability of the quantities to be measured becomes quite concealed by the unavoidable errors of observation, the proposition, that the limit of the residual magnetism corresponds to a finite value of the magnetizing force, must be qualified by adding that, with further refinement of our means of observation, this limit will be removed more and more towards zero, and perhaps at length vanish entirely. With this qualification, the results obtained may serve as an experimental confirmation of Maxwell's theory of residual magnetism. Meanwhile I will just mention here that the theory obtains another important argument in its favour through the results of experiments to be hereafter communi-

cated, so that the correctness of its hypotheses has become almost a certainty. But on the ground of these experiments I also remark beforehand that then only can we properly speak of a limit of the residual magnetism, when we presuppose the steel freshly annealed and *perfectly unmagnetic*. A body even only a little permanently magnetic shows another limit of the residual magnetism, varying according to the amount of it. A *perfectly unmagnetic* piece of steel, however, belongs to the order of ideal notions; and therefore it will be impossible to *demonstrate* Maxwell's assumption by actual experiment (see § 9).

I refrain from discussing the values of k , because I hold them for the most part to possess too little accuracy to deduce conclusions from them, or even to compare them with those ascertained by means of the earth-inductor.

Thinking that the magnetic state of the ellipsoids could only have been very little altered by the forces hitherto applied, I now subjected the pieces of sort I. once more to induction by the vertical component of the earth's magnetism. As a proof of the correctness of this assumption, the following values of k were obtained, which do not sensibly differ from those given in Table II. :—

I. 1.	15·592
2.	11·596
3.	10·702
4.	10·004
5.	8·965

§ 8. The ellipsoids were now subjected successively to increasing forces magnetizing always in the same direction, which continually raised the residual moment to greater values.

Wiedemann* has already remarked that, when a steel bar is for the first time magnetized by galvanic currents, the temporary moments generated in it during the action of the currents increase at first more quickly than the intensity of the currents, and only later commence to approach a maximum. This behaviour should also take place after repeated magnetizations and demagnetizations of the bars on again magnetizing them.

Now the experiments show that no change in this behaviour occurs even when the bar is not cleared of its residual magnetism after every observation, but this is continually augmented by ascending forces. Let a series of experiments with ellipsoid II. 4 serve for an example. X denotes the magnetizing force of the helix, TM the temporary moment, RM the total moment remaining after the cessation of the action, con-

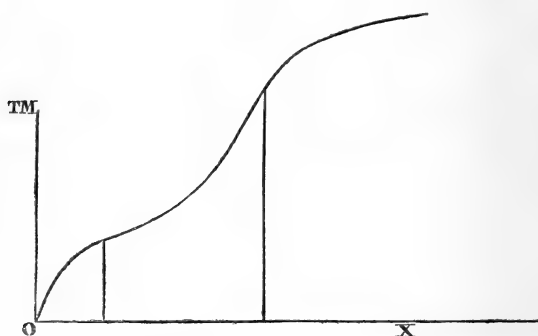
* *Galvanismus*, 2nd ed. vol. ii. p. 350.

sequently the initial moment (UM) increased by that generated by the force.

X.	$\frac{TM}{X}$.	$\frac{RM}{TM}$.
13.32	103600	46.7
22.82	103800	41.0
71.31	107800	23.8
94.65	108700	17.7
111.2	109100	15.9

While, therefore, the temporary moments increase more quickly than the magnetizing forces, the residual again increase more quickly than the temporary. Hence it follows also that the residual moments increase more rapidly than the magnetizing forces, and according to the product of the ratios $\frac{TM}{X}$ and $\frac{RM}{TM}$.

If we now collect all the results relating to the temporary magnetism of steel, we find that it increases at first more slowly, then more quickly, and finally (as other observers have already found) again more slowly than does the magnetizing force; consequently a graphic representation of the dependence of the temporary magnetism on the magnetizing force must give a curve of the following form:—



The two ordinates drawn correspond to the two turning-points of the magnetization.

[To be continued.]

XXV. *On the Formulation of the Paraffins and their Derivatives.* By WILLIAM ODLING, *M.B., F.R.S.**

A SERIES of hydrocarbons, of composition and unit-weight expressible by the formulæ CH_4 , C_2H_6 , C_3H_8 , C_4H_{10} , C_5H_{12} , &c. respectively, constitutes the series of the paraffins (H. Watts). The first member of the series, CH_4 or H_4C , is familiarly known as marsh-gas; and the series itself is sometimes spoken of as the marsh-gas series.

Each successive member of the series differs in composition from the immediately preceding member by an increment of carbo-hydrogen, CH_2 ; and as, in the unit-weight of every member of the series, the number of proportions of hydrogen exceeds by two proportions twice the number of proportions of carbon, the series itself, as well as each member of it, is expressible by the general formula $\text{C}_n\text{H}_{2n+2}$.

Excluding the earlier terms, the members of the paraffin series are noteworthy for the number of their isomeric modifications. Thus there are two known paraffins expressible by the formula C_4H_{10} , three by the formula C_5H_{12} , four or five by the formula C_6H_{12} or H_{12}C_6 , &c.

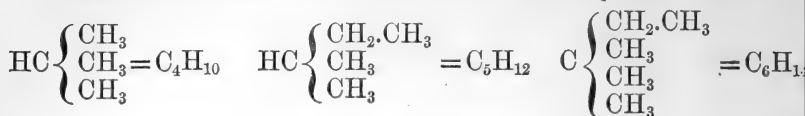
The paraffins are met with principally as products of destructive distillation, but are obtainable also by various definite reactions. And, in particular, a large number of them have been produced, directly or indirectly, by the coalescence of two or more marsh-gas residues, as set forth below.

From a study of the modes of formation and transformation of certain members of the paraffin series, it results that every paraffin may be regarded as producible by the coalescence of two or more marsh-gas residues, in such a way that each particular residue coalesces with as many other residues as it is itself deficient in the full number of proportions of hydrogen proper to marsh-gas, H_4C or CH_4 .

Thus, of the four residues, methyl, H_3C or CH_3 , methylene, H_2C or CH_2 , formyl, HC or CH , and carbon, C , the residue methyl coalesces with one other residue only, whether tri-, di-, mono-, or anhydric; while the residue methylene coalesces with two, the residue formyl with three, and the residue carbon with four other residues, whether alike or different, and whether anhydric, monohydric, or polyhydric respectively.

The notion of these various ways of coalescence is expressed in the following fully developed formulæ for some of the paraffins,—coalescence of the different residues being implied by the lateral contiguity of their respective symbols :—

* Communicated by the Author.



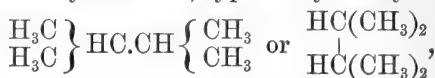
With the increase in the number of marsh-gas residues coalescing to furnish a paraffin, the modes of their conceivable arrangement, subject to the above-mentioned condition, become almost infinitely various. In all paraffins at present known, however, the modes of arrangement are reducible to four (Schorlemmer); and the paraffins are accordingly divisible into four subclasses, namely:—a dimethyl subclass, typified by the hydro-

carbon $\text{H}_2\text{C} \left\{ \begin{array}{l} \text{CH}_3 \\ \text{CH}_3 \end{array} \right.$ or $\text{H}_2\text{C}(\text{CH}_3)_2$; a trimethyl subclass, typified

by the hydrocarbon $\text{HC} \left\{ \begin{array}{l} \text{CH}_3 \\ \text{CH}_3 \\ \text{CH}_3 \end{array} \right.$ or $\text{HC}(\text{CH}_3)_3$; a tetramethyl

subclass, typified by the hydrocarbon $\text{C} \left\{ \begin{array}{l} \text{CH}_3 \\ \text{CH}_3 \\ \text{CH}_3 \\ \text{CH}_3 \end{array} \right.$ or $\text{C}(\text{CH}_3)_4$; and

another tetramethyl subclass, typified by the hydrocarbon

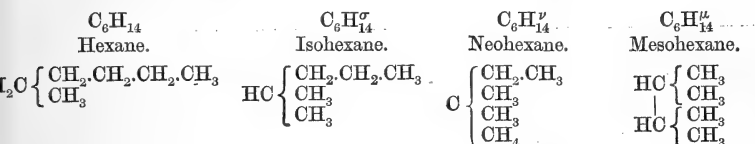


the perpendicular hyphen or line used in this last formula serving to indicate the coalescence of the two formyl residues written one under the other.

Variously constituted penta-, hexa-, and yet more highly methylated paraffins are readily conceivable, but have not actually been met with. Moreover the known members of the two tetramethylated subclasses are but few and comparatively unimportant.

The dimethylated and trimethylated paraffins have been distinguished for some time past as normal and isoparaffins respectively; and it being useful to have distinctive names for the other two subclasses, the subclass typified by the hydrocarbon $\text{C}(\text{CH}_3)_4$ may be conveniently designated as that of neo- or latest paraffins, and the subclass typified by the hydrocarbon $\text{HC}(\text{CH}_3)_3$ as that of mesoparaffins,—the prefix meso- suggesting by its sound a special connexion between this and the

iso-subclass, and by its meaning the intermediate nature of this subclass, as associated with that of the isoparaffins in being formylic, and with that of the neoparaffins in being tetramethylic. Taking then the Greek letters σ , ν , and μ as indices of the iso-, neo-, and meso-subclasses, the four best-known hexanes, for instance, may be expressed by the following condensed formulæ respectively :—



In the majority of cases, the grouped marsh-gas residues $\text{CH}_2 \cdot \text{CH}_3$, $\text{CH}_2 \cdot \text{CH}_2 \cdot \text{CH}_3$, $\text{CH}_2 \cdot \text{CH}_2 \cdot \text{CH}_2 \cdot \text{CH}_3$, and their homologues, or ethyl, propyl, butyl, &c., are preferably represented by the familiar condensed expressions C_2H_5 , C_3H_7 , C_4H_9 , &c.; and employing these expressions, the several normal, iso- and neoparaffins will appear as di-, tri-, and tetra-derivatives respectively of methane or marsh-gas, and the several meso-paraffins as tetra-derivatives of ethane, propane, and the succeeding homologues of marsh-gas, as exemplified in the following Table (p. 208) of formulæ for the 3-, 4-, 5-, 6-, and 7-carbon paraffins.

With regard to the setting out of the formulæ of the normal paraffins, it is to be observed that the formula given for pentane, for instance, $\text{H}_2\text{C} \left\{ \begin{array}{l} \text{C}_3\text{H}_7 \\ \text{C} \text{ H}_3 \end{array} \right.$ or propyl-methyl methane, is only one condensed form of the expression



and that, so far as the nature of the paraffin is concerned, other condensed forms of the expression, as $\text{H}_3\text{C} \cdot \text{C}_4\text{H}_9$ or butyl-methane, $\text{H}_5\text{C}_2 \cdot \text{C}_3\text{H}_7$ or propyl-ethane, $\text{H}_2\text{C} \left\{ \begin{array}{l} \text{C}_2\text{H}_5 \\ \text{C}_2\text{H}_5 \end{array} \right.$ or diethyl-methane, and $\text{H}_6\text{C}_3 \left\{ \begin{array}{l} \text{CH}_3 \\ \text{CH}_3 \end{array} \right.$ or dimethyl-propane, are just as correct, and are each of them, in view of certain reactions and relations, even more convenient. But upon the whole, the mode of setting out the formulæ of the normal paraffins made use of in the Table appears to be most generally advantageous, from its best illustrating the relationship of the normal to the iso- and neoparaffins, and more particularly, as will hereafter be seen, from its best illustrating the relationship of the normal paraffins both to their derived ketones and secondary alcohols, and also to their associated olefins. This mode of expression,

PARAFFINS.

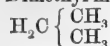
ISOPARAFFINS.

NEOPARAFFINS.

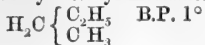
MESOPARAFFINS.

Propane, C_3H_8 .

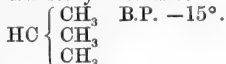
Dimethyl methane.

*Butanes*, C_4H_{10} .

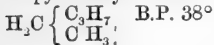
Ethyl-methyl methane.



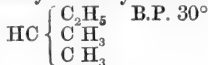
Trimethyl methane.

*Pentanes*, C_5H_{12} .

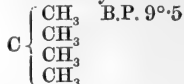
Propyl-methyl methane.



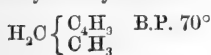
Ethyl-dimethyl methane.



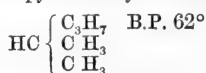
Tetramethyl methane.

*Hexanes*, C_6H_{14} .

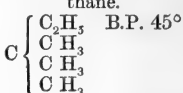
Butyl-methyl methane.



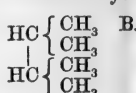
Propyl-dimethyl methane.



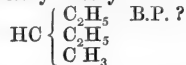
Ethyl-trimethyl methane.



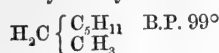
Tetramethyl methane.



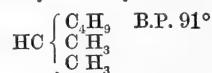
Diethyl-methyl methane.

*Heptanes*, C_7H_{16} .

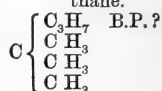
Pentyl-methyl methane.



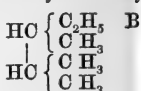
Butyl-dimethyl methane.



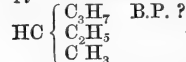
Propyl-trimethyl methane.



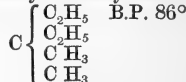
Ethyl-trimethyl methane.



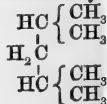
Propyl-diethyl methane.



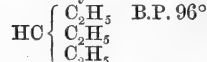
Diethyl-dimethyl methane.



Tetramethyl methane.



Triethyl methane.



however, must not be taken to imply that in the normal paraffins one proportion of methyl stands in a different relation to the other proportion, or any one proportion of methylene in a different relation to the remaining proportions.

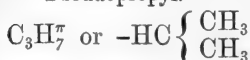
By the direct or indirect replacement in any paraffin, of one proportion of hydrogen by a proportion of chlorine or other radical, simple or compound, a great variety of mono-derivatives of each of the several paraffins is obtainable. Representing the proportion of radical introduced in the place of one proportion of hydrogen by the letter X, the mono-derivatives of propane, for instance, will be expressible by the formula C_3H_7X , those of butane and isobutane by the formulæ C_4H_9X and $C_4H_9^oX$ respectively; those of pentane, isopentane, and neopentane by the formulæ $C_5H_{11}X$, $C_5H_{11}^oX$, and $C_5H_{11}^{\prime}X$ respectively, and so forth; the residues C_3H_7 , C_4H_9 , $C_4H_9^o$, C_5H_{11} , $C_5H_{11}^o$, $C_5H_{11}^{\prime}$, &c. constituting the paraffin- or alcohol- monad radicals propyl, butyl, isobutyl, pentyl, isopentyl or amyl, and neopentyl, &c.

But it is found that in any single paraffin, the replacement of one proportion of hydrogen by one proportion of some particular radical—chlorine, Cl, or hydroxyl, OH, for instance—gives rise to the production of at least two distinct isomers. And a study of the formative and transformative reactions of these isomers leads to the conclusion that the difference between them depends upon whether they result from a substitution of the introduced radical for the hydrogen of a methyl or of a methylene residue (in butane for instance, $H_2C \begin{cases} CH_2 \cdot CH_3 \\ CH_3 \end{cases}$), or for the hydrogen of a methyl or of a formyl residue (in isobutane for instance, $HC(CH_3)_3$).

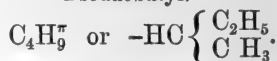
The paraffin radicals resulting in this way from the introduction of a foreign radical in the place of the hydrogen of a methyl- or of a methylen- or of a formyl- residue in the original paraffin, are spoken of as primary, secondary, and tertiary radicals respectively. To the root-names of the secondary and tertiary radicals, moreover, it is convenient to add some distinctive prefix; and the prefix "iso" has been very generally made use of, not only in the case of the primary radicals of isoparaffins, but also in the case of the secondary and tertiary radicals generally, though necessarily with much resulting confusion. Occasionally, however, the prefix "pseudo" has been applied in the case of the secondary radicals—a practice it seems desirable to employ more systematically; and, taking the Greek letter π for the index (ψ being reserved for the rarer case of the secondary radicals of the isoparaffins), the

secondary radicals pertaining to propane and butane, for instance, will be named and formulated as follows :—

Pseudopropyl.



Pseudobutyl.

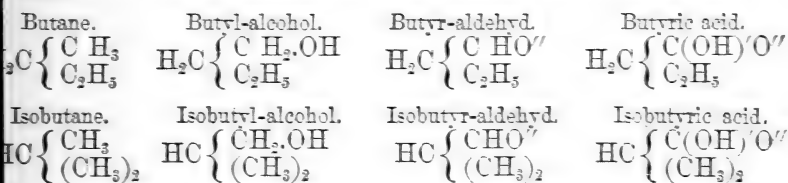


Similarly in the case of the tertiary radicals, the prefix “kata” and the index letter κ may well be resorted to,—the latter recalling to memory that in these radicals the carbon (κ) of the characteristic marsh-gas residue alone remains unreplaced. The tertiary 4-carbon radical, for instance, will be formulated as $C_4H_9^\kappa$ or $-C(CH_3)_3$, and be spoken of as katabutyl. The following Table of 2-, 3-, 4-, and 5-carbon radicals, primary, secondary, and tertiary, is drawn up in accordance with these suggestions.

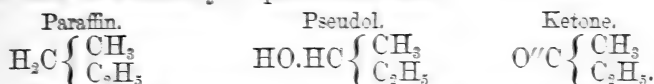
PARAFFINS. PRIMARY RADICALS. PSEUDO-RADICALS. KATA-RADICALS.

Propane. $H_2C \left\{ \begin{array}{l} CH_3 \\ CH_3 \end{array} \right.$	Propyl. $H_2C \left\{ \begin{array}{l} CH_2X \\ CH_3 \end{array} \right.$	Pseudopropyl. $XHC \left\{ \begin{array}{l} CH_3 \\ CH_3 \end{array} \right.$	
Butane. $H_2C \left\{ \begin{array}{l} C_2H_5 \\ C H_3 \end{array} \right.$	Butyl. $H_2C \left\{ \begin{array}{l} C_2H_4X \\ C H_3 \end{array} \right.$	Pseudobutyl. $XHC \left\{ \begin{array}{l} C_2H_5 \\ C H_3 \end{array} \right.$	
Isobutane. $HC \left\{ \begin{array}{l} CH_3 \\ CH_3 \\ CH_3 \end{array} \right.$	Isobutyl. $HC \left\{ \begin{array}{l} CH_2X \\ CH_3 \\ CH_3 \end{array} \right.$		Katabutyl. $XO \left\{ \begin{array}{l} CH_3 \\ CH_3 \\ CH_3 \end{array} \right.$
Pentane. $H_2C \left\{ \begin{array}{l} C_3H_7 \\ C H_3 \end{array} \right.$	Pentyl. $H_2C \left\{ \begin{array}{l} C_3H_6X \\ C H_3 \end{array} \right.$	Pseudopentyls. $XHC \left\{ \begin{array}{l} C_3H_7 \\ C H_3 \end{array} \right.$ $XHC \left\{ \begin{array}{l} C_2H_5 \\ C_2H_5 \end{array} \right.$	
Isopentane. $HC \left\{ \begin{array}{l} C_2H_5 \\ C H_3 \\ C H_3 \end{array} \right.$	Isopentyls. $HC \left\{ \begin{array}{l} C_2H_4X \\ C H_3 \\ C H_3 \end{array} \right.$ $HC \left\{ \begin{array}{l} C_2H_5 \\ C H_3 \\ C H_2X \end{array} \right.$	Pseudisopentyl. $HC \left\{ \begin{array}{l} CHX.CH_3 \\ CH_3 \\ CH_3 \end{array} \right.$	Katapentyl. $XO \left\{ \begin{array}{l} C_2H_5 \\ C H_3 \\ C H_3 \end{array} \right.$
Neopentane. $C \left\{ \begin{array}{l} CH_3 \\ CH_3 \\ CH_3 \\ CH_3 \end{array} \right.$	Neopentyl. $C \left\{ \begin{array}{l} CH_2X \\ CH_3 \\ CH_3 \\ CH_3 \end{array} \right.$		

The compounds resulting from the paraffins through the substitution, for one proportion of hydrogen in the paraffin, of one proportion of hydroxyl (that is, of oxygen half-saturated by hydrogen) constitute the paraffin alcohols, hydrates, or carbinols, $C_n H_{2n+1} OH$. Unlike the paraffins, the alcohols are readily susceptible of oxidation, and, according to their several natures, are found to undergo oxidation in three different fashions. Treated with oxidizing agents, the alcohols of one description first lose two proportions of hydrogen, H_2 , to furnish a compound of well-characterized properties known as an aldehyde,—this aldehyde then acquiring one proportion of oxygen, O , to furnish an acid, and this acid further undergoing a disruptive oxidation only. Alcohols of this description, or alcohols proper, are found to result from the replacement of the hydrogen of a methyl residue of the original paraffin by hydroxyl, and are known therefore as primary alcohols :—



Alcohols of another description, when treated with the same oxidizing agents, first lose two proportions of hydrogen, H_2 , to furnish a compound of well-characterized properties known as a ketone, this ketone further undergoing a disruptive oxidation only. Alcohols of this description are found to result from the replacement of the hydrogen of a methylene residue of the original paraffin by hydroxyl, and belong therefore to the class of secondary or pseudo-alcohols.

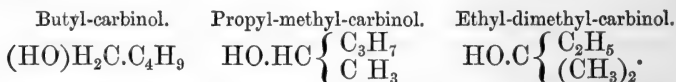


Alcohols of yet a third description, when treated with the same oxidizing agents, yield neither the succession of aldehyd and acid nor yet a ketone, but suffer a disruptive oxidation only. Alcohols of this description, in some respects allied to the phenols, are found to result from the replacement of the hydrogen of a formyl residue of the original paraffin by hydroxyl, and are known, therefore, as tertiary alcohols :—



The only system of nomenclature for the alcohols at present much in use is that devised by Kolbe. The basis of this sys-

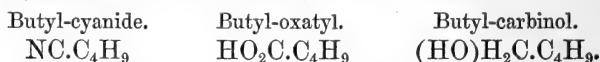
tem consists in specializing by the name of carbinol that particular marsh-gas residue, whether mono-, di-, or trihydric in which a replacement of hydrogen by hydroxyl has been effected, and in naming therewith the associated hydrocarbon residue or residues of the original paraffin. In this way the primary, secondary, and tertiary 5-carbon alcohols, for instance, are designated by Kolbe as follows, his mode of formulating them, however, being different from that employed below :—



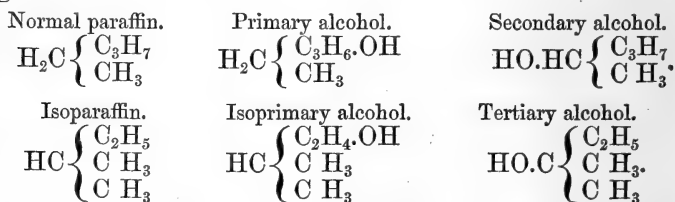
It was pointed out, when speaking of the paraffins, that the expression for normal pentane, for instance, $\text{H}_2\text{C} \left\{ \begin{array}{l} \text{C}_3\text{H}_7 \\ \text{C} \text{ H}_3 \end{array} \right.$, is equally with other expressions, including $\text{H}_3\text{C}.\text{C}_4\text{H}_9$, merely a condensation of the expression



Now it is obviously from the expression $\text{H}_3\text{C}.\text{C}_4\text{H}_9$ for the paraffin that the above expression $(\text{HO})\text{H}_2\text{C}.\text{C}_4\text{H}_9$ for the primary alcohol is derived ; and this mode of formulation may often be resorted to with advantage in the case of primary alcohols, from its bringing out so well the relationship between them and the secondary and tertiary alcohols, as shown in the above examples—and also the relationship to one another of the cyanide, acid, and alcohol, as shown below :—

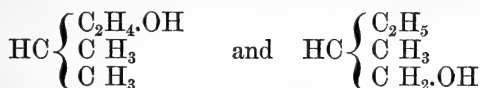


On the other hand, it fails to indicate the derivation of the secondary alcohol from the normal paraffin, and that of the tertiary alcohol from the isoparaffin, as illustrated by formulæ of the description thought to be, on the whole, most advantageous :—



It is further to be remarked that the several expressions $(\text{HO})\text{H}_2\text{C}.\text{C}_4\text{H}_9$ and $\text{H}_3\text{C}.\text{C}_4\text{H}_8(\text{OH})$, $\text{H}_2\text{C} \left\{ \begin{array}{l} \text{C} \text{ H}_2.\text{OH} \\ \text{C}_3\text{H}_7 \end{array} \right.$,

$\text{H}_2\text{C} \left\{ \begin{array}{l} \text{C}_3\text{H}_6 \cdot \text{OH} \\ \text{C} \text{ H}_3 \end{array} \right.$, and $\text{H}_2\text{C} \left\{ \begin{array}{l} \text{C}_2\text{H}_4 \cdot \text{OH} \\ \text{C}_2\text{H}_5 \end{array} \right.$, for the normal primary alcohol are really identical, being but different condensations of the one formula, $\text{CH}_2(\text{OH}) \cdot \text{CH}_2 \cdot \text{CH}_2 \cdot \text{CH}_2 \cdot \text{CH}_3$, but that the expressions $\text{HO} \cdot \text{HC} \left\{ \begin{array}{l} \text{C}_3\text{H}_7 \\ \text{C} \text{ H}_3 \end{array} \right.$, and $\text{HO} \cdot \text{HC} \left\{ \begin{array}{l} \text{C}_2\text{H}_5 \\ \text{C}_2\text{H}_5 \end{array} \right.$ for the normal secondary alcohol, and the expressions

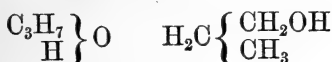


for the isoprimary alcohol are radically different, and applicable in each case to a different compound.

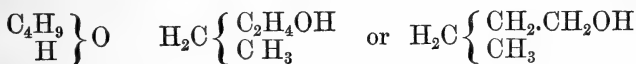
Taking the precaution to express in all cases the hydroxylated marsh-gas residue as a separate constituent of the alcohol, Kolbe's system of nomenclature is quite in accordance with the system of formulation for the paraffins and paraffin radicals previously explained and exemplified. It seems preferable, however, to restrict the application of the term carbinol in great measure to the tertiary alcohols, to accord the term pseudo-alcohol or pseudol to the secondary alcohols, and to employ the term alcohol specifically to designate the alcohols proper or primary alcohols, which, looked upon as hydrocarbon derivatives of the type $\text{XCH}_2(\text{OH})$, may also be called methols.

ALCOHOLS PROPER, OR PRIMARY ALCOHOLS.

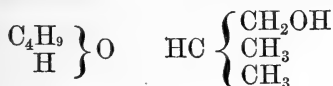
Propyl alcohol, or Ethyl methol, or carbinol, $\text{H}_5\text{C}_2 \cdot \text{CH}_2\text{OH}$. B.P. 97° .



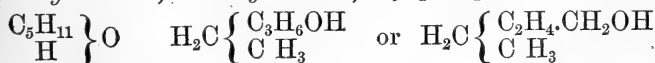
Butyl alcohol, or Propyl methol, $\text{H}_7\text{C}_3 \cdot \text{CH}_2\text{OH}$. B.P. 116° .



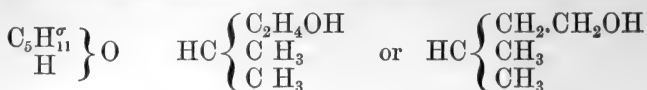
Isobutyl alcohol, or Pseudopropyl methol, $\text{H}_7\text{C}_3^\pi \cdot \text{CH}_2\text{OH}$. B.P. 109° .



Pentyl alcohol, or Butyl methol, $\text{H}_9\text{C}_4 \cdot \text{CH}_2\text{OH}$. B.P. 137° .



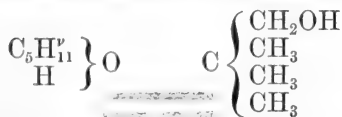
Isopentyl, i. e. *Amyl alcohol*, or *Isobutyl methol*, $\text{H}_9\text{C}_4^\sigma.\text{CH}_2\text{OH}$. B.P. 13°



β *Isopentyl alcohol*, or *Pseudobutyl methol*, $\text{H}_9\text{C}_4^\pi.\text{CH}_2\text{OH}$. B.P. ?



Neopentyl alcohol, or *Katabutyl methol*, $\text{H}_9\text{C}_4^\kappa.\text{CH}_2\text{OH}$. B.P. ?



PSEUDO-ALCOHOLS, PSEUDOLS, OR SECONDARY ALCOHOLS.

Pseudopropyl alcohol, or *Dimethyl carbinol* or *pseudol*. B.P. 83°.



Pseudobutyl alcohol, or *Ethyl-methyl pseudol*. B.P. 99°.



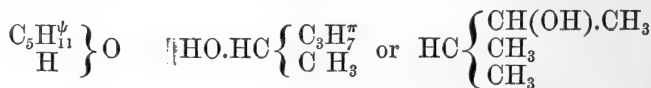
Pseudopentyl alcohol, or *Propyl-methyl pseudol*. B.P. 118°.



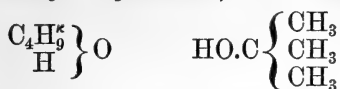
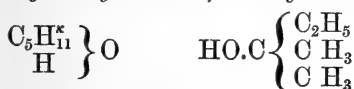
β *Pseudopentyl alcohol*, or *Di-ethyl pseudol*. B.P. ?



Pseudisopentyl alcohol, or *Pseudopropyl-methyl pseudol*. B.P. 104°

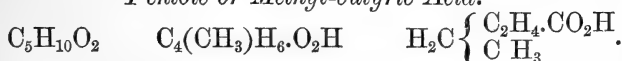
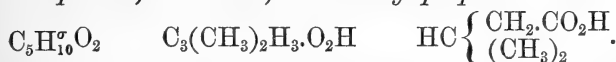
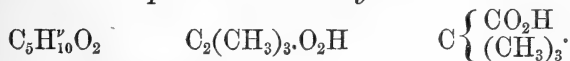


CARBOLS, CARBINOLS PROPER, OR TERTIARY ALCOHOLS.

Tertiary Butyl alcohol, or Trimethyl carbinol. B.P. 82°·5.*Tertiary Pentyl alcohol, or Ethyl-dimethyl carbinol.* B.P. 98°·5.

In the above Tables the first column shows the condensed formulæ of the several 2-, 3-, 4-, and 5-carbon alcohols as water-type derivatives, and the second column their formulæ as paraffin derivatives. In the third column, a few of these last formulæ are set out in a slightly more elaborated form; and those of the primary alcohols thereby adapted to Kolbe's system of names, which, however, appear of less advantage as applied to this class of primary than as applied to either of the other two classes of alcohols.

The system of alcohol notation employed in these Tables has the advantage of manifesting clearly the relationship of the paraffin alcohols in general to their associated paraffins and to the olefins and olefin alcohols, also the relationship of the primary alcohols to their associated aldehyds and fatty acids, and of the secondary alcohols to their associated ketones, and, lastly, the relationship of the alcohols in general to their associated glycols, glycerines, and erythrites, &c. Its adaptability to the fatty acids corresponding to the several primary alcohols, whether derived from normal, iso-, or neoparaffins, &c., is illustrated below. The three recognized 5-carbon acids, for instance, may be designated and formulated as follows, the normal acid being further capable of formulation as

*Pentoic or Methyl-butyril Acid.**Isopentoic, or Valeric, or Dimethyl-propionic Acid.**Neopentoic or Trimethyl-acetic Acid.*

As regards the choice of distinctive names, indicative of the modes of origin and transformation of the different isomeric acids, those chemists who employ Kolbe's system of nomenclature in the case of the primary alcohols will naturally prefer to use some corresponding terms for their associated acids, and, calling the alcohol butyl-carbinol or -methol, for instance, would call the associated acid butyl-oxatyl or derivatively as butyl-formic acid, thus:—

	Butyl	Isobutyl	Katabutyl
-carbinols .	$(\text{HO})\text{H}_2\text{C}.\text{C}_4\text{H}_7$	$(\text{HO})\text{H}_2\text{C}.\text{C}_4\text{H}_7^r$	$(\text{HO})\text{H}_2\text{C}.\text{C}_4\text{H}_7^s$
-formic acids	$\text{HO}_2\text{C}.\text{C}_4\text{H}_7$	$\text{HO}_2\text{C}.\text{C}_4\text{H}_7^r$	$\text{HO}_2\text{C}.\text{C}_4\text{H}_7^s$

Other chemists, again, paying special regard to the constituent residues of the different acids, will prefer to designate the three 5-carbon acids, for instance, as ethyl-propionic, pseudopropyl-acetic, and katabutyl-formic acids respectively. Altogether, however, the names which call to mind that the normal, iso-, and neo-acids are mono-, di-, and trimethyl &c. derivatives would seem to be most advantageous. The relationship to one another of the different series of names is shown below:—

Normal, Iso-, and Neo-pentonic Acids, C₅H₁₀O₂.

Nor. <i>Methyl-butyric.</i>	Ethyl-propionic	Propyl-acetic	Butyl-formic
Iso-	<i>Dimethyl-propionic.</i>	Pseudoprop-acetic	Isobutyl-formic
Neo-		<i>Trimethyl-acetic.</i>	Katabutyl-formic

The simplest term of the iso-series is derivable from the secondary alcohol or pseudol of the next preceding lower carbon group, and the simplest term of the neo-series from the tertiary alcohol or carbinol of its next preceding lower carbon group,—in each case by an indirect substitution of oxatyl for hydroxyl, thus:—

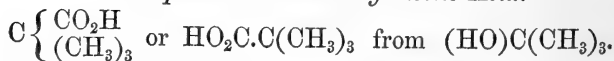
Normal Propionic or Methyl-acetic Acid.



Isobutyric or Dimethyl-acetic Acid.

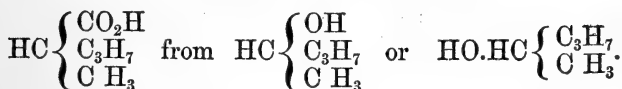


Neopentonic or Trimethyl-acetic Acid.

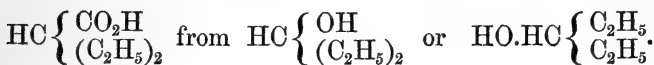


Where, however, as in the case of the more complex succeeding terms, isomerism does or may occur among iso-acids with one another, and among neo-acids with one another, these relations of derivation are confined to certain isomeric forms only, as instanced in the case of the four acknowledged forms of isocaproic acid, the first two forms only being derivable as above from the two 5-carbon pseudols each to each, and the last two deriving from the two 5-carbon iso-primary alcohols each to each, by the substitution, as before, of oxatyl for hydroxyl, thus:—

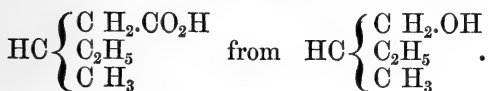
Propyl-methyl-acetic Acid.



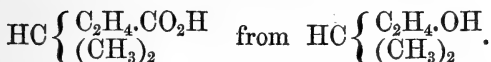
Diethyl-acetic Acid.



Ethyl-methyl-propionic Acid.



Dimethyl-butyric Acid.



Similarly, as regards the derivation of the succeeding from the preceding alcohols by indirect exchange of $\text{CH}_2(\text{OH})$ for OH , it may be noticed in the Table of alcohols proper that while the first isoalcohol or isobutyl alcohol is derivable, as above, from the pseudopropyl or first secondary alcohol, but one of the two conceivable isopentyl alcohols would be derivable in this way from pseudobutyl alcohol, the other being derived instead from primary isobutyl alcohol.

XXVI. *On the Bearing of the Fundamental Law of Electrodynamics toward the Principle of the Conservation of Energy, and on a further Simplification of the former.* By Professor R. CLAUSIUS*.

THE new fundamental law of electrodynamics recently communicated by me† presents, in relation to its admissibility and the still existing possibility of a simplification, occasion for some very essential considerations, which I take leave likewise to communicate.

Let two particles of electricity in motion, e and e' , have at the time t the rectangular coordinates x, y, z and x', y', z' , and for abbreviation put $\xi = x - x', \eta = y - y', \zeta = z - z'$. Further, let r denote the distance of the two particles one from the other, ds and ds' two path-elements simultaneously traversed by them, ϵ the angle between these, and v and v' the velocities. If then the components, in the coordinate-directions, of the force which the particle e' exerts upon the particle e is represented by Xee', Yee', Zee' , equations are valid to which in my previous communication I gave the more general form

$$X = \frac{\xi}{r^3} - k \left(\frac{\xi}{r^3} \cos \epsilon + n \frac{d^2 \xi}{ds ds'} \right) vv' + k \frac{d}{dt} \left(\frac{1}{r} \frac{d\xi}{dt} \right),$$

$$Y = \frac{\eta}{r^3} - k \left(\frac{\eta}{r^3} \cos \epsilon + n \frac{d^2 \eta}{ds ds'} \right) vv' + k \frac{d}{dt} \left(\frac{1}{r} \frac{d\eta}{dt} \right),$$

$$Z = \frac{\zeta}{r^3} - k \left(\frac{\zeta}{r^3} \cos \epsilon + n \frac{d^2 \zeta}{ds ds'} \right) vv' + k \frac{d}{dt} \left(\frac{1}{r} \frac{d\zeta}{dt} \right),$$

where k is the constant which refers to the quantitative ratio between the electrodynamic and the electrostatic force, and n denotes a constant provisionally left undetermined.

The question now arises, whether the law of force expressed by these equations is compatible with the principle of the Conservation of Energy.

If the electrodynamic action on each other of the two particles is effected through a material medium existing between them, it is not absolutely necessary that the forces to which the two individual particles are subjected shall by themselves

* Translated from a separate impression, communicated by the Author, having been laid before the Niederrheinische Gesellschaft für Natur- und Heilkunde on the 7th Feb. 1876.

† Phil. Mag. January 1876, pp. 69-71.

alone satisfy that principle, since the material medium also takes part in the action. For the actions exerted upon one another by closed galvanic currents, however, we may, in accordance with their known laws, expect to find that principle satisfied even without taking account of the presence of a material substance between them.

If now the above expressions of X , Y , and Z be multiplied successively by $\frac{dx}{dt}$, $\frac{dy}{dt}$, and $\frac{dz}{dt}$, and likewise the expressions which are formed in a corresponding manner for the components X' , Y' , and Z' of the forces acting on the particle e be multiplied by $\frac{dx'}{dt}$, $\frac{dy'}{dt}$, and $\frac{dz'}{dt}$, and then the whole be added and the total multiplied by the product ee' and the time-element dt , we shall obtain the expression of the work done during this time-element by the two forces. This expression, if for the present the terms affected by the factor n be omitted, can be brought into the following form,

$$-d \frac{ee'}{r} [1 - k(v^2 + v'^2 - vv' \cos \epsilon)] - \frac{k}{2} \frac{ee'}{r} d(v^2 + v'^2).$$

The first term in this is a complete differential, thus corresponding with the principle of the Conservation of Energy, while the second term does not fulfil this condition.

But let us now consider two galvanic current-elements, which may move in any way whatever and can be variable in their intensity, we have then to admit that in each of these elements an equal quantity of positive and negative electricity is present. Let us denote these quantities by $+e$, $-e$, $+e'$, $-e'$, and combine $+e$ with $+e'$, $+e$ with $-e'$, $-e$ with $+e'$, and $-e$ with $-e'$; we have then for each of these four combinations to construct an expression of the foregoing form, and to add up the four expressions. Thereby we obtain from the last term (which by taking away the brackets divides into two), in the whole, eight terms, each two of which are equal and opposite, so that collectively they annul one another. The sum then consists therefore of only the four terms corresponding to the first term of the foregoing expression, which, as before said, satisfies the principle of the Conservation of Energy.

In regard to the terms affected by the factor n , above omitted, these likewise, in the expression of the work which refers to two single current-elements, are in part eliminated; and those that remain become *nil* by integration with respect to closed currents.

Accordingly the above equations are, as required by the

facts established by experiment, in harmony with the principle of the Conservation of Energy.

I have already, in my previous communication, said that it is theoretically most probable that the value of the constant n is zero. Thereby the terms affected by the factor n fall away of themselves; and consequently the principle of the Conservation of Energy is fulfilled not merely for closed currents, but also for the single elements of them.

Besides this simplification, however, a second can now be introduced, which likewise refers only to a constituent of the formulæ not affecting the actions of a closed galvanic current.

I have, in deriving the above equations, already deviated in some essential points from the views hitherto held. I have, namely, taken into consideration not merely the relative motion of the two particles of electricity, but also their absolute motions; and for the electrodynamic forces exerted by the particles on one another I have neglected the assumption that their direction must fall in the line joining the particles. On the other hand, I have held fast to the hypothesis that the two forces are equal and opposed to one another. Nevertheless even this hypothesis is not necessary for forces such as the electrodynamic; if we drop it also, we can give to the fundamental equations the following form:—

$$X = -\frac{d}{dx} \frac{1}{r} (1 - k v v' \cos \epsilon) - k \frac{d}{dt} \left(\frac{1}{r} \frac{dx'}{dt} \right);$$

$$Y = -\frac{d}{dy} \frac{1}{r} (1 - k v v' \cos \epsilon) - k \frac{d}{dt} \left(\frac{1}{r} \frac{dy'}{dt} \right);$$

$$Z = -\frac{d}{dz} \frac{1}{r} (1 - k v v' \cos \epsilon) - k \frac{d}{dt} \left(\frac{1}{r} \frac{dz'}{dt} \right).$$

The force acting on the particle e , determined by these equations, and the corresponding force acting on the particle e' satisfy by themselves alone the principle of the Conservation of Energy; for the work done by them during an element of time is represented by the following complete differential,

$$-d \frac{ee'}{r} (1 + k v v' \cos \epsilon).$$

By employing a method introduced by Lagrange on another occasion, the individual force-components can also be expressed

more simply. If, namely, we put

$$U = \frac{ee'}{r},$$

$$V = \frac{ee'}{r} vv' \cos \epsilon,$$

$$= k \frac{ee'}{r} \left(\frac{dx}{dt} \frac{dx'}{dt} + \frac{dy}{dt} \frac{dy'}{dt} + \frac{dz}{dt} \frac{dz'}{dt} \right),$$

and regard U as a function of the six coordinates x, y, z, x', y', z' , and V as a function of these six coordinates and their differential-coefficients according to t , we can write

$$X_{ee'} = \frac{d(V-U)}{dx} - \frac{d}{dt} \left(\frac{dV}{d \frac{dx}{dt}} \right);$$

and in just the same manner can the other five force-components be derived from the two functions U and V by differentiation.

For the components of the force which is exerted upon a galvanic current-element ds by a current-element ds' we get from the simplified form of the fundamental equations the following expressions:—

$$civ' ds ds' \left(\frac{d \frac{1}{r}}{dx} \cos \epsilon - \frac{d \frac{1}{r}}{ds} \frac{dx'}{ds'} \right);$$

$$civ' ds ds' \left(\frac{d \frac{1}{r}}{dy} \cos \epsilon - \frac{d \frac{1}{r}}{ds} \frac{dy'}{ds'} \right);$$

$$civ' ds ds' \left(\frac{d \frac{1}{r}}{dz} \cos \epsilon - \frac{d \frac{1}{r}}{ds} \frac{dz'}{ds'} \right).$$

XXVII. *On the Relative Values of the Pieces in Chess.* By H. M. TAYLOR, M.A., Fellow and Tutor of Trinity College, Cambridge*.

THE object of this paper is to ascertain the relative values of the pieces on a chessboard. If a piece be placed on a square of a chessboard, the number of squares it commands depends in general on its position. If we calculate the average number of squares which any particular piece commands

* Communicated by the Author.

when placed in succession on every square of the board, it seems fair to assume that this gives a not very inexact measure of the value of the piece.

For special reasons the above problem is stated in the following manner :—"A king and a piece of different colours are placed at random on two squares of a chessboard of n^2 squares : it is required to find the chance that the king is in check."

The ordinary chessboard has an even number of squares ; and as some of the results take different forms for odd and even values of n , the results are given merely for even values of n , and the results for the ordinary chessboard of 64 squares deduced from them.

As the relative values of the knight and bishop on the ordinary chessboard on this hypothesis came out in a ratio very different from the ratio that is ordinarily received by chess-players, it occurred to the author to investigate the chance that when a king and a piece of different colours were placed at random on two squares of a board, the king would be in check but unable to take the piece. This check is called *safe* check in contradistinction to a mere check, which may be safe or unsafe and which is called *simple* check.

Simple check from one rook.

A rook in any position checks $2(n-1)$ squares. The king can be placed on n^2-1 for any given position of the rook. The chance of check, therefore, is

$$\frac{2(n-1)}{n^2-1} = \frac{2}{n+1}.$$

If $n=8$,

the chance = $\frac{2}{9}$.

Safe check from one rook.

a	b	b	
b	c	c	
b	c	c	

If the rook be on a corner square, it could be taken by a king in check on two squares, and so on. The number of safe checks by a rook on the different squares is given by the following scheme :—

Rook on	Number of safe checks.	Number of such positions of the rook.
<i>a</i>	$2n-4$	4
<i>b</i>	$2n-5$	$4(n-2)$
<i>c</i>	$2n-6$	$(n-2)^2$

The chance

$$= \frac{4(2n-4) + 4(n-2)(2n-5) + (n-2)^2(2n-6)}{n^2(n^2-1)} = \frac{2(n-2)}{n(n+1)}.$$

When $n=8$,

$$\text{the chance} = \frac{1}{6}.$$

Simple check with one knight.

The number of squares attacked by a knight placed on any square of a chessboard is given by the following scheme :—

<i>a</i>	<i>b</i>	<i>c</i>	<i>c</i>	
<i>b</i>	<i>c</i>	<i>d</i>	<i>d</i>	
<i>c</i>	<i>d</i>	<i>e</i>	<i>e</i>	
<i>c</i>	<i>d</i>	<i>e</i>	<i>e</i>	

Knight on	Number of checks.	Number of such positions of knight.
<i>a</i>	2	4
<i>b</i>	3	8
<i>c</i>	4	$4(n-3)$
<i>d</i>	6	$4(n-4)$
<i>e</i>	8	$(n-4)^2$

The chance of check

$$\frac{2 \cdot 4 + 3 \cdot 8 + 4 \cdot 4(n-3) + 6 \cdot 4(n-4) + 8(n-4)^2}{n^2(n^2-1)} = \frac{8(n-2)}{n^2(n+1)}.$$

If $n=8$,

$$\text{chance} = \frac{1}{12}.$$

For a knight all checks are *safe* checks.

Simple check with one bishop.

We will at first assume that a bishop can be put on either a

white or a black square. The number of squares attacked by a bishop on any square of the chessboard is given by the following scheme :—

<i>a</i>	<i>a</i>	<i>a</i>	<i>a</i>	<i>a</i>	
<i>a</i>	<i>b</i>	<i>b</i>	<i>b</i>	<i>b</i>	
<i>a</i>	<i>b</i>	<i>c</i>	<i>c</i>		
<i>a</i>	<i>b</i>	<i>c</i>	<i>d</i>		

Bishop on		Number of checks.	Number of such positions of bishop.
<i>a</i>	$n-1$	$4(n-1)$
<i>b</i>	$n+1$	$4(n-3)$
<i>c</i>	$n+3$	$4(n-5)$
<i>d</i>	$n+5$	$4(n-7)$
.	.	.	.

If n be even, we have $\frac{n}{2}$ lines of this scheme all obeying the same law ; if n be odd, we have $\frac{n-1}{2}$ such lines, and another for the middle square of the board for which the number of checks $= 2(n-1)$.

Now m terms of the series

$$\begin{aligned}
 & (n-1)(n-1) + (n+1)(n-3) + (n+3)(n-5) + \&c. \\
 & = n^2 - 2n + 1 + n^2 - 2n - 1 \cdot 3 + n^2 - 2n - 3 \cdot 5 + \&c. \\
 & \quad + n^2 - 2n - (2m-3)(2m-1) \\
 & = m(n^2 - 2n) + 1 - (1 \cdot 3 + 3 \cdot 5 + 5 \cdot 7 + \dots \\
 & \quad + (2m-3)(2m-1)) \\
 & = m(n^2 - 2n) + 1 - \frac{(2m-3)(2m-1)(2m+1) + 3}{6}.
 \end{aligned}$$

Put $m = \frac{n}{2}$, then the numerator of the chance-fraction

$$\begin{aligned}
 & = 4 \left\{ \frac{n}{2}(n^2 - 2n) + 1 - \frac{1}{6}(n-3)(n-1)(n+1) - \frac{1}{2} \right\} \\
 & = \frac{2n}{3}(n-1)(2n-1).
 \end{aligned}$$

Put $m = \frac{n-1}{2}$, then the numerator of the chance-fraction

$$= 4 \left\{ \frac{n-1}{2} (n^2 - 2n) + 1 - \frac{(n-4)(n-2)n}{6} - \frac{1}{2} \right\} + 2(n-1)$$

$$= \frac{2n}{3} (n-1)(2n-1).$$

The chance, therefore, for both odd and even values of n

$$= \frac{2 \cdot \frac{n}{3} (n-1)(2n-1)}{n^2(n^2-1)} = \frac{2}{3} \cdot \frac{2n-1}{n(n+1)}.$$

If $n=8$,

$$\text{the chance} = \frac{5}{36}.$$

If the bishop be restricted to squares of one and the same colour, white or black, then if n be even the numerator and the denominator of the chance-fraction are both halved, and the chance is the same as before. If n be odd, we have two cases to consider. If we take the squares of the same colour as the centre square, the numerator of the chance-fraction is twice $\frac{n-1}{2}$ terms of the series

$$(n-1)(n-1) + (n+1)(n-3) + (n+3)(n-5) + \&c.$$

increased by $2(n-1)$,

$$= \frac{n-1}{3} (2n^2 - n + 3).$$

If we take the squares of the other colour, the numerator of the chance-fraction is twice the $\frac{n-1}{2}$ terms of the above series

$$= \frac{(n-1)(n+1)(2n-3)}{3}.$$

Now, when a bishop occupies one square, the king can be placed on any one of n^2-1 squares. Therefore the chance of check when the bishop is restricted to squares of the same colour as the middle one

$$= \frac{\frac{n-1}{3} (2n^2 - n + 3)}{\frac{n^2-1}{2} \cdot (n^2-1)} = \frac{2}{3} \cdot \frac{2n^2 - n + 3}{(n+1)(n^2+1)}.$$

When the bishop is restricted to squares of the other colour, the chance of check

$$= \frac{\frac{(n-1)(n+1)(2n-3)}{3}}{\frac{n^2-1}{2} \cdot (n^2-1)} = \frac{2}{3} \cdot \frac{2n-3}{n^2-1}.$$

Safe check with one bishop.

This is best found by finding the number of unsafe checks. It will be seen that if the bishop be on a corner square, there is one unsafe check; for any other outside square there are two unsafe checks; and for any other square there are four. The whole number therefore of unsafe checks

$$= 4 + 4(n-2) \cdot 2 + (n-2)^2 \cdot 4 = 4(n^2 - 4n + 4 + 2n - 4 + 1) \\ = 4(n-1)^2.$$

Therefore the numerator of the chance-fraction

$$= \frac{2n}{3}(n-1)(2n-1) - 4(n-1)^2 = \frac{2}{3}(n-1)(n-2)(2n-3),$$

and the chance of safe check

$$= \frac{\frac{2}{3}(n-1)(n-2)(2n-3)}{n^2(n^2-1)} = \frac{2}{3} \cdot \frac{(n-2)(2n-3)}{n^2(n+1)}.$$

If $n=8$,

$$\text{the chance} = \frac{1}{144}.$$

These results will be the same for a bishop restricted to squares of the same colour on a board of an even number of squares.

Simple check with one queen.

The queen on any square of a chessboard checks all the squares that a bishop on the same square would, as well as all the squares that a rook on the same square would. The numerator of the chance-fraction, therefore, will be the sum of the numerators for the cases of the rook and the unrestricted bishop. The chance, therefore, for the queen is the sum of the chances for the bishop and rook

$$= \frac{2}{3} \cdot \frac{2n-1}{n(n+1)} + \frac{2}{n+1} = \frac{2}{n+1} \cdot \frac{(2n-1)+3n}{3n} = \frac{2}{3} \cdot \frac{5n-1}{n(n+1)}.$$

If $n=8$,

$$\text{the chance} = \frac{1}{36}.$$

Safe check with one queen.

The number of unsafe checks with one queen is seen thus: if the queen be on a corner square, there are 3 unsafe checks, if on any other outside square there are 5, and if on any other square there are 8 unsafe checks. The total number of unsafe checks, therefore, is

$$4 \cdot 3 + 4(n-2) \cdot 5 + (n-2)^2 \cdot 8 = 4(n-1)(2n-1).$$

The numerator of the chance-fraction, therefore, is

$$\frac{2}{3}n(n-1)(2n-1) + n^2 \cdot 2(n-1) - 4(2n-1)(n-1) \\ = \frac{2(n-1)(5n-3)(n-2)}{3}.$$

Therefore the chance

$$= \frac{\frac{2}{3}(n-2)(n-1)(5n-3)}{n^2(n^2-1)} = \frac{2}{3} \frac{(n-2)(5n-3)}{n^2(n+1)}.$$

If $n=8$,

$$\text{the chance} = \frac{37}{144}.$$

Simple check with two bishops.

We will assume that n is an even number, and that one bishop (A) is restricted to white squares, and the other (B) to black squares. The number of squares checked by the two bishops for all different positions of B when A is on a particular square is obtained from the following scheme :—

a	a'	a	a'
a'	b	b'	b
a	b'	c	c'
a'	b	c'	d

A on a square on the $\left(\frac{n}{2}-r\right)$ th row from the outside.

Number of such squares.	B on	Number of such positions of B.	Number of squares checked by A and B.
$2(2r-1)$	a	$2(n-1)$	$3n-2r-2$
	b	$2(n-3)$	$3n-2r$
	c	$2(n-5)$	$3n-2r+2$
	d	$2(n-7)$	$3n-2r+4$

The total number of checks for all positions of B while A remains fixed

$$= 2(n-1)(3n-2r-2) + 2(n-3)(3n-2r) + 2(n-5)(3n-2r+2) + 2(n-7)(3n-2r+4) + \&c. \text{ to } \frac{n}{2} \text{ terms,}$$

$$= 2(n-1)((2n-2r-3) + n+1) + 2(n-3)((2n-2r-3) + n+3) + 2(n-5)((2n-2r-3) + n+5) + \&c. \text{ to } \frac{n}{2} \text{ terms,}$$

$$= 2 \cdot \frac{n^2}{4} \cdot (2n-2r-3) + 2 \left(\frac{n}{2} \cdot n^2 - \frac{n(n^2-1)}{6} \right)$$

$$= -rn^2 + \frac{n}{6}(10n^2-9n+2).$$

This result must be multiplied by $2(2r-1)$, the number of the positions of A on the given row, and the product summed for values of r between 1 and $\frac{n}{2}$. The numerator of the chance-fraction, therefore,

$$\begin{aligned}
 &= \sum_{r=1}^{\frac{n}{2}} (4r-2) \left(-rn^2 + \frac{n(10n^2-9n+2)}{6} \right) \\
 &= -4n^2 \cdot \sum r^2 + \left\{ \frac{2n}{3} (10n^2-9n+2) + 2n^2 \right\} \sum r - \frac{n}{2} \cdot \frac{n}{3} (10n^2-9n+2) \\
 &= -4n^2 \cdot \frac{\frac{n}{2} \left(\frac{n}{2} + 1 \right) (n+1)}{6} + \frac{\frac{n}{2} \left(\frac{n}{2} + 1 \right)}{2} \cdot \left(\frac{2n}{3} (10n^2-9n+2) + 2n^2 \right) - \frac{n^2}{6} (10n^2-9n+2) \\
 &= \frac{n^3}{6} (4n^2-6n+2).
 \end{aligned}$$

The chance, therefore,

$$\begin{aligned}
 &= \frac{\frac{n^3}{6} (4n^2-6n+2)}{\frac{n^2}{2} \cdot \frac{n^2}{2} \cdot (n^2-2)} \\
 &= \frac{4}{3} \cdot \frac{(n-1)(2n-1)}{n(n^2-2)}.
 \end{aligned}$$

If $n=8$,

$$\text{the chance} = 1\frac{35}{124}.$$

Simple check from two rooks.

We will call the rooks A and B. A can be placed on n^2 squares, B on n^2-1 squares for each position of A, and the king on n^2-2 squares for each position of A and B. If A and B defend each other, they check altogether $3n-4$ squares, and there are $2n-2$ squares on which B will defend A on a given square. If B does not defend A, they check $4n-6$ squares, and there are $(n-1)^2$ squares on which B will not defend A on a given square. The chance of the king being in check

$$= \frac{n^2(3n-4)(2n-2) + n^2(4n-6)(n-1)^2}{n^2(n^2-1)(n^2-2)} = \frac{2(2n^2-2n-1)}{(n+1)(n^2-2)}.$$

If $n=8$,

$$\text{the chance} = \frac{37}{93}.$$

Chance of the king being in check.

	For chessboards of n^2 squares (n even).		For ordinary chessboard of 64 squares.	
	Simple.	Safe.	Simple.	Safe.
Knight	$\frac{8(n-2)}{n^2(n+1)}$	$\frac{8(n-2)}{n^2(n+1)}$	$\frac{1}{12}$	$\frac{1}{12}$
Bishop	$\frac{2}{3} \cdot \frac{2n-1}{n(n+1)}$	$\frac{2}{3} \cdot \frac{(n-2)(2n-3)}{n^2(n+1)}$	$\frac{5}{36}$	$\frac{13}{144}$
Rook	$\frac{2}{n+1}$	$\frac{2(n-2)}{n(n+1)}$	$\frac{2}{9}$	$\frac{1}{6}$
Queen	$\frac{2}{3} \cdot \frac{5n-1}{n(n+1)}$	$\frac{2}{3} \cdot \frac{(n-2)(5n-3)}{n^2(n+1)}$	$\frac{13}{36}$	$\frac{37}{144}$
Two bishops.	$\frac{4}{3} \cdot \frac{(n-1)(2n-1)}{n(n^2-2)}$		$\frac{35}{124}$	
Two rooks...	$\frac{2(2n^2-2n-1)}{(n+1)(n^2-2)}$		$\frac{37}{93}$	

We may conclude by remarking that the relative values of the knight, bishop, rook, and queen are, according as we measure them by the chance of simple check or of safe check, on the ordinary chessboard in the ratio of 3, 5, 8, 13, or 12, 13, 24, 37 respectively ; while the values of the pieces in the same order, as given by Staunton in the ‘Chess-Player’s Handbook,’ are 3.05, 3.50, 5.48, and 9.94, the value of the pawn being taken as unity.

N.B.—The value of a pawn depends so much on the fact that it is possible to convert it into a queen, that this method does not appear applicable to it.

XXVIII. On a new Form of Wave-apparatus suitable for the Lecture-room. By C. J. WOODWARD, B.Sc.*

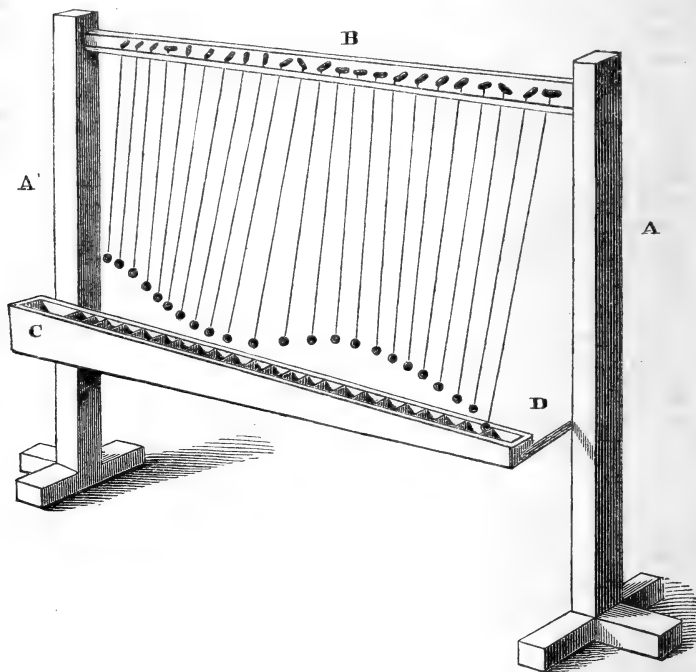
THE apparatus about to be described illustrates the motion of the air-particles when a sound-wave is propagated, and also the motion of the æther molecules in a wave of plane-polarized light.

Wave motion consists in the repetition by a number of particles of some prescribed motion which is given to the first of the particles and taken up successively by the others, a certain

* Communicated by the Physical Society.

interval elapsing between the successive motions. If, for example, we have a number of pendulums of equal length hanging from a beam, but their lower ends supported so that they are out of the vertical, we shall find on releasing one of them, say the left-hand end one, it will commence swinging to and fro; by the time the first pendulum has gone over one fourth of its path release the second, and then after equal intervals the third, fourth, &c., and a wave motion will be produced. If each pendulum was drawn aside in a line at right angles to the beam from which they are suspended, a wave corresponding to a ray of polarized light will be produced; but if each had been drawn aside in the same plane as that of the beam, a wave corresponding to that of a ray of sound would be obtained. We require, then, in order to illustrate these waves, merely a row of pendulums and some arrangement for releasing them in succession.

The apparatus I have used is represented in the figure. A, A



Plane wave produced by placing the pendulums in the compartments of the box C and releasing them in succession by lowering the box. In the figure the last ball is about leaving the box.

are two uprights about 10 feet high, with a crosspiece B fastened firmly to them. At intervals of 3 inches along this crosspiece are inserted violin-pegs, each peg carrying a light string with a heavy ball of 1 inch diameter attached. Before fastening the strings to the violin-pegs, each string is passed through a small hole in the projecting base of the crosspiece, so that the length of the pendulum is determined by the distance of the ball from the base of the crosspiece. C is a light wooden box deeper at one end than at the other, with a number of compartments corresponding to the number of pendulums. The box C is attached at the ends to two radial arms, one of which, D, is seen in the figure. Cords are also attached to C running over pulleys, by which the box C may be raised and lowered. These cords would confuse the figure if introduced; and their action can be easily realized.

Suppose it be wished to illustrate a plane wave. The box C is raised, and the pendulums are placed one in each compartment so that they are all drawn on one side. By means of the cords the box C is gently lowered, when the pendulums are successively released and the wave formed.

To illustrate the sound-wave, the box C is removed from the radial arms and is hung vertically underneath the crosspiece B; one or two of the pendulums (to the left, say) are removed, and the others placed in the compartments as before; only now each pendulum, instead of being drawn from under the crosspiece, lies still underneath, but to the left of the vertical line let fall from each point of support. On now allowing the box to fall slowly, the pendulums are released successively and a wave of condensation and rarefaction is formed.

In conclusion I should acknowledge the assistance of Mr. H. F. Marshall, of Birmingham, who has rendered great service in simplifying the mechanical arrangements of the apparatus.

Birmingham and Midland Institute,
January 1876.

XXIX. *Notices respecting New Books.*

Théorie des Formes Binaires par le Chev. F. FÀA DE BRUNO, Docteur ès Sciences de l'Université de Paris, Professeur à l'Université de Turin. Turin: Libraire Brero, 1876 (8vo, pp. 320, with Appendices).

THIS work contains the substance of the author's lectures delivered at the University of Turin, and is designed to put within the reach of students a knowledge of the Modern Higher Algebra, or the Algebra of Linear Transformations. It has so far the same aim as Dr. Salmon's 'Lessons introductory to the Modern

Higher Algebra;" the work before us, however, discusses the properties of binary forms only, *i. e.* the properties of homogeneous functions of two variables x and y , and does not enter upon the more general question of functions of three or more variables. After four preliminary chapters on the Symmetrical Functions of the Roots of Equations, on Resultants (*i. e.* Eliminants, the result of eliminating x between two equations), on Discriminants, and on Canonical Forms, the author discusses the properties of Invariants and Covariants, which are the main subject of his work. If we take the form

$$a_0x^2 + 2a_1xy + a_2y^2, \quad . \quad . \quad . \quad . \quad . \quad (1)$$

and subject it to linear transformation by substituting $pX + qY$ for x , and $p'X + q'Y$ for y , it becomes

$$A_0X^2 + 2A_1XY + A_2Y^2;$$

we shall now find that

$$A_0A_2 - A_1^2 = (pq' - p'q)^2(a_0a_2 - a_1^2);$$

so that the function $a_0a_2 - a_1^2$ of the coefficients of (1) undergoes no change in the transformation beyond being multiplied by the square of the determinant of the transformation, *viz.* $(pq' - p'q)^2$; consequently the function $a_0a_2 - a_1^2$ of the coefficients of (1) is called an invariant of that form. If we take cubic, quartic, or other forms, functions of their coefficients can be found which have the same property of invariancy.

When forms of higher degrees than the third are considered, it is found that they have several invariants, which in terms of the coefficients of the form are long expressions. In fact the invariant (ϕ) of the r th degree of a binary form of the n th degree can be expressed as the sum of all products of powers of the coefficients such as

$$Ca_0^{m_0}a_1^{m_1} \dots a_n^{m_n},$$

where

$$0 \cdot m_0 + 1 \cdot m_1 + \dots + nm_n = \frac{1}{2}nr,$$

and

$$m_0 + m_1 + \dots + m_n = r;$$

the indices and suffixes must be taken to satisfy these equations in every possible way; and then the constants (C) can be determined by the relation

$$a_0 \frac{d\phi}{da_1} + 2a_1 \frac{d\phi}{da_2} + \dots + na_{n-1} \frac{d\phi}{da_n} = 0.$$

It is plain from this statement that, if r and n are moderately large numbers, the invariant will consist of a large number of terms. Thus in the case of the quintic form ($n=5$) the invariant of the fourth degree ($r=4$) consists of the sum of twelve terms while the invariant of the 18th degree ($n=5, r=18$) consists of 848 terms.

It may be inferred from this that the subject branches rapidly out into a variety of complicated developments. Thus we see that

by giving different values to r different invariants can be obtained for a given form; and then the questions arise, are they independent of each other? if not, which are the fundamental invariants, and in what way are the others derived from them? *E. g.* in the case of the quintic form ($n=5$) there are four fundamental invariants, from which the others can be deduced by a comparatively simple process; the four, however, are not independent of each other, for it is found that the square of the invariant of the 18th degree can be expressed in terms of those of the 4th, 8th, and 12th degrees.

An enormous extension of the subject is due to the fact that when a form (ϕ) is given, it is possible to assign a second form (ψ) whose coefficients are connected with those of the given form in such a manner that, when both undergo linear transformation, the same relation exists between the coefficients of the transformed functions as between the coefficients of the functions in their original shape; ϕ and ψ are called covariants. In the work before us the subject of Covariants occupies more than twice the space devoted to Invariants, though the limitation to Binary forms is strictly observed.

It is this limitation which will render the work of great value to students, as it enables the author to give a complete exposition of the elements of the subject. To use his own words:—"There is nothing better for the studious reader who wishes to make progress than to have recourse to the original memoirs themselves; and this he will be able to do with safety and advantage when he has thoroughly learned all that I have expounded in the present work" (p. vi). Within the assigned limits the subject is worked out with great completeness: thus, not only are the properties of Symmetrical Functions of the Roots of Equations demonstrated and the means by which their calculation can be expedited explained, but in an Appendix the results of the actual calculation up to the eleventh degree are given. In a second Appendix are given the invariants of forms up to the fifth degree, expressed both as functions of the coefficients and as functions of the roots, and in an abridged shape those of forms of the sixth degree. In the same Appendix the covariants are given of forms from the third up to the sixth degree. It would seem that M. de Bruno has performed all the needful calculations himself, though some at least of the results had been published without his being aware of it at the time his calculations were made. In short no pains have been spared to render the work as complete as possible; and, indeed, it appears from an incidental remark (p. vi) that the author must have had the work on hand for something like twenty years.

The notation employed is of necessity complicated; and this, while it makes misprints misleading to the reader, makes them also hard to avoid. Though the book is printed in a good type and has altogether a good appearance, we cannot help fearing that a minute examination would yield a large crop of errata; at all events we have come across several which are not noticed in the list at the

end of the volume; on pp. 52, 53 there are certainly five misprints, as well as one or two other points that apparently need alteration.

M. de Bruno in many places gives notices of the history of the subject; and this renders a mistake in the very first page of the Preface matter for regret. The passage runs as follows:—"La Théorie des Invariants . . . doit son origine à un excellent mémoire de Boole sur les Transformations linéaires inséré dans le Cambridge and Dublin Mathematical Journal, 1847." The article in question consists of two parts, both in the third volume of the 'Cambridge Mathematical Journal'; and the first part was published in November 1841. The 'Cambridge and Dublin Journal' was a second series of the Cambridge Journal. There were four volumes of the latter published. The twofold mistake in the date and the name of the Journal has the effect of quite misrepresenting the history of the origin of the subject, though it can only be due to an oversight, as the means of correcting it are to be found within the volume itself.

XXX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 159.]

March 11, 1875.—Joseph Dalton Hooker, C.B., President, in the Chair.

THE following paper was read:—

"On the Absorption-Spectra of Metals volatilized by the Oxyhydrogen Flame." By J. Norman Lockyer, F.R.S., and W. Chandler Roberts, Chemist of the Mint.

The researches which have recently been published on the absorption-spectra of various metals, first by Roscoe and Schuster and subsequently by one of us*, establish beyond all question the facts that—

I. In addition to the well-known line-spectra, channelled-space spectra are produced by the vapours of certain metals; and,

II. Such spectra are produced by vapours which are competent to give at other times, not only line-spectra, but continuous spectra in the blue, or blue and red.

As the temperature employed for the volatilization of the metals in the experiments to which we have referred did not exceed bright redness, or that at which cast iron readily melts, the range of metals examined was necessarily limited. We have therefore considered it desirable to extend these observations to the less fusible metals, as well as to ascertain whether the spectra of those which were volatilized at the lower temperature would be modified by the application of a greater degree of heat. For this purpose we have employed the flame of an oxyhydrogen blowpipe. This instrument

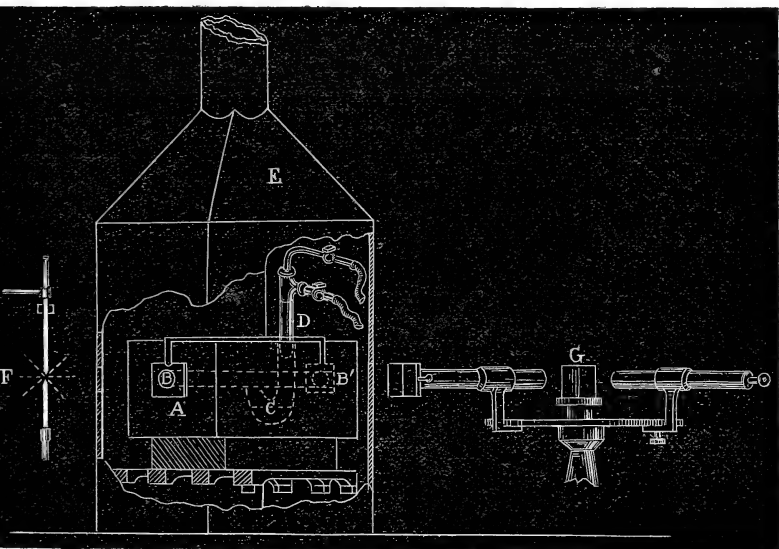
* Proc. Roy. Soc. vol. xxii. pp. 362 and 371 respectively.

devised by Sainte-Claire Deville and Debray*, renders it possible to attain high temperatures with great facility, and Stas has already employed their method in the distillation of silver†. The lime still arranged by him has been modified in that about to be described, in order that the metallic vapour might be conducted into a lime tube or tunnel heated to whiteness, so placed that a beam from an electric lamp could readily traverse it.

Description of the Apparatus and method of Manipulation.

The apparatus employed is shown in the figure, in which A is the block of lime ‡ divided horizontally by a plane through the axis of the tube (B B'), this tube being 16 centims. long and 30 millims. diameter. The receptacle (C) communicates with the centre of B B' and is open at the upper surface of the lime block, in order to admit of the introduction of the oxyhydrogen blowpipe (D), which is provided with a thick nozzle of platinum 20 millims. in diameter. The ends of the tunnel in the lime were closed by glass plates held on by a suitable clip. Small lateral orifices were cut in the lime for the insertion of tobacco-pipe stems, through which a stream of hydrogen could be passed into the tube and receptacle.

An electric lamp (F), in connexion with a 30-cell Bunsen's



* Ann. de Chimie et de Physique, tom. lvi. p. 413.

† Stas, 'Sur les lois des proportions Chimiques,' p. 56.

‡ We are indebted to the well-known metallurgist, Mr. J. S. Sellon, of the firm of Johnson and Matthey, for a pure variety of limestone from which the blocks were prepared, and it answered its purpose admirably.

battery, was placed opposite one end of the tube, and a spectro-scope (G) opposite the other. This last instrument was by Desaga, of Heidelberg, and its single prism, the angle of which was 60° , was capable of distinctly separating the D lines, at the same time that it enabled us to see the whole spectrum in a single field of view, an essential point in such inquiries. The magnifying-power of the telescope was 7.5 linear.

Some preliminary experiments indicated the advisability of increasing the length of the column of vapour. To effect this, a tube 30 centims. long was made in a fresh block of lime, the cavity being arranged as before; in each end a short accurately fitting iron tube, luted with a mixture of graphite and fireclay, was inserted; and the total length of the column thus became 60 centims.

The lime block (C) with its fittings was then placed in the charcoal-furnace (E), by means of which the whole could be raised to a high temperature. As soon as the block was heated to bright redness, the metal, the vapour of which was to be examined, was introduced into the cavity (C), and the flame of the oxyhydrogen blowpipe (D) was allowed to play on its upper surface, care being taken to employ an excess of hydrogen. In almost every case the metal experimented on was rapidly volatilized (the exceptions being gold and palladium). The central portion of the lime block was raised to a white heat by the action of the blowpipe. As the glass plates rapidly became clouded by the condensation of the metallic vapours, it was necessary to adopt an arrangement by which they could be easily replaced. We may state that, among the precautions which we adopted in order to assure ourselves that oxides were not present to disturb the accuracy of the results, one of the glass plates was removed at the conclusion of each experiment, and the presence of an excess of hydrogen conclusively proved by igniting it at the open end.

We were enabled at any time, by modifying the conditions of the gas-supply, to introduce the spectrum of the oxyhydrogen flame. It may further be stated that, with few exceptions, the metals were previously melted in a stream of hydrogen and enclosed, until experimented on, in sealed glass tubes. We ascertained that the effect of oxides, and of the metallic rain due to condensation, was to produce a general absorption obviously different from the special effects of absorption which we now proceed to record.

DETAILS OF THE EXPERIMENTS.

Silver.—Fifty grammes of pure metal were placed in the cavity (C); and this amount produced a continuous supply of vapour for about 10 minutes.

With the smaller thickness given by the first lime block, and with a less powerful blast, the spectrum of silver consisted of an absorption in the blue which at times extended almost to the green.

With the elongated tube and a stronger blast an exquisite channelled-space absorption was observed, the channels being far enough

apart to render them very conspicuous in the field of view; at the same time there was continuous absorption in the blue. It was specially observed that there was no absorption in the red.

It may be interesting to note that the vapour of silver when condensed into fine particles, escaping into an atmosphere of hydrogen, is blue by reflected light.

Copper.—With the greatest thickness only a continuous absorption in the blue could be obtained.

Sodium.—Only the dark D line was observed, no traces of channelled-space absorption being visible.

Calcium.—We operated upon a small piece of metal prepared by the late Dr. Matthiessen, but no result was obtained.

Aluminium.—When the temperature was so high that the spectrum of the flame was visible, an absorption was suspected in the violet; and the appearance did not change on one glass end being removed.

Zinc.—Many experiments were made on this metal; but there are several points connected with it which require further investigation, and we therefore reserve our remarks on the spectrum of zinc for a future occasion.

Cadmium.—Under both conditions of thickness the vapour of cadmium gave, in the blue only, an absorption which was very decided; an absorption in the red was also noticed which had not been observed in previous experiments when a low temperature was employed.

Manganese.—A small quantity of this metal was prepared with great care by Mr. Bayly, one of the assistant assayers, and it gave a distinct absorption in the red and blue, with evidences of a channelled-space spectrum. In a repetition of the experiment a more distinct channelled-space spectrum was observed.

Iron.—The metal employed had been obtained by electro-deposition in the manner suggested by Mr. Jacobi. Its vapour gave a slight continuous absorption in the blue.

Cobalt also gave a slight continuous absorption in the blue, but less than in the case of iron.

Nickel.—This metal behaved in the same manner as cobalt, the absorption being about equal in amount.

Chromium.—The amount of metal volatilized was very small, but a fine channelled-space spectrum was observed.

Tin.—This metal caused a considerable absorption in the blue, but less in the red, no traces of a channelled-space spectrum being visible.

Antimony.—In results already published it is stated that at the low temperature antimony gives a channelled-space spectrum. In the present experiments we observed merely absorption in the blue; and this is the only case in which the effects at a high temperature were inferior to those at a low temperature. As the purity of the metal first employed may be doubted, little reliance can be placed on these exceptional results.

Bismuth.—With the greatest thickness the absorption of bismuth

is strikingly similar to that of iodine at a dull red heat. We have first a bank of continuous absorption in the blue with a sharp boundary on the less refrangible side, and then a channelled-space absorption throughout the entire green part of the spectrum reaching to D.

Lead.—This metal at first caused an absorption at both ends of the spectrum; shortly afterwards the whole spectrum was extinguished. As this is a readily oxidizable metal, special care was taken to prove that a large excess of hydrogen was present.

Thallium.—We are indebted to Mr. Crookes for a generous supply of this metal. The characteristic green line of thallium was observed *bright*, the light of the arc not being reversed; and it may be interesting to note that the vapour of this metal was incandescent five minutes after the withdrawal of the flame.

Gold.—A distinct absorption in the blue and red was observed, but there were certainly no traces of a channelled-space spectrum. The spectral lines due to the oxyhydrogen flame were very conspicuous. It may be noted that the amount of gold volatilized was only 0.01 oz.; but this quantity of metal was sufficient to produce an abundant supply of vapour.

Palladium.—This metal caused a distinct absorption in the blue, but no effect was noticed at the red end of the spectrum. There was no channelled-space spectrum, and the lines caused by the oxyhydrogen flame were barely visible.

Selenium.—With the greatest thickness employed a channelled-space spectrum was given by selenium.

Iodine.—It will be remembered that, according to the results already published by one of us, iodine vapour exhibits, at a low temperature, a channelled-space spectrum, and a bank of absorption in the violet. These later experiments showed that, at the more elevated temperature, this bank was broken up and disappeared, leaving a continuous channelled-space spectrum.

These experiments, made at the Royal Mint, were often prolonged for many hours consecutively. They involved much furnace-work of a peculiarly trying nature; and we have much pleasure in acknowledging the assistance we received from Mr. Edward Rigg, one of the assistant assayers, who conducted many of the tedious manipulations with great skill and patience. We should also mention that the care exercised by Joseph Groves, senior fireman, in the preparation of the furnace and the lime-moulds, contributed in no small measure to the success of the experiments.

It appears to us that these experiments, conducted at the high temperature of the oxyhydrogen flame, go far to support the conclusions which were drawn from the experiments at a lower temperature. First, in passing from the liquid to the most perfect gaseous state, vapours are composed of molecules of different orders of complexity; and second, this complexity is diminished by the dissociating action of heat, each molecular simplification being marked by a distinctive spectrum. There is also an intimate connexion between the facility with which the final stage is reached,

the group to which the element belongs, and the place which it occupies in the solar atmosphere.

April 8.—Joseph Dalton Hooker, C.B., President, in the Chair.

The following paper was read:—

“Experiments to ascertain the Cause of Stratification in Electrical Discharges *in vacuo*.” By Warren De La Rue, Hugo W. Müller, and William Spottiswoode.

Some results obtained in working with a chloride-of-silver battery of 1080 cells in connexion with vacuum-tubes appear to be of sufficient interest to induce us to communicate them to the Society, in anticipation of the more detailed account of an investigation which is now being prosecuted, and which it is intended to continue, shortly, with a battery of 5000 cells, and possibly with a far greater number.

The battery used up till now consists of 1080 cells, each being formed of a glass tube 6 inches (15·23 centims.) long and $\frac{3}{4}$ of an inch (1·9 centim.) internal diameter; these are closed with a vulcanized rubber stopper (cork), perforated eccentrically to permit the insertion of a zinc rod, carefully amalgamated, $\frac{3}{16}$ (0·48 centim.) of an inch in diameter and 4·5 inches (11·43 centims.) long. The other element consists of a flattened silver wire passing by the side of the cork to the bottom of the tube and covered, at the upper part above the chloride of silver and until it passes the stopper, with thin sheet gutta percha for insulation, and to protect it from the action of the sulphur in the vulcanized corks; these wires are $\frac{1}{16}$ of an inch (0·16 centim.) broad and 8 inches (20·32 centims.) long. In the bottom of the tube is placed 225·25 grains (14·59 grms.) chloride of silver in powder; this constitutes the electrolyte: above the chloride of silver is poured a solution of common salt containing 25 grammes chloride of sodium to 1 litre (1752 grains to 1 gallon) of water, to within about 1 inch (2·54 centims.) of the cork. The connexion between adjoining cells is made by passing a short piece of india-rubber tube over the zinc rod of one cell, and drawing the silver wire of the next cell through it so as to press against the zinc. The closing of the cells by means of a cork prevents the evaporation of water, and not only avoids this serious inconvenience, but also contributes to the effectiveness of the insulation. The tubes are grouped in twenties in a sort of test-tube rack, having four short ebonite feet, and the whole placed in a cabinet 2 ft. 7 in. (78·74 centims.) high, 2 ft. 7 in. wide, and 2 ft. 7 in. deep, the top being covered with ebonite to facilitate working with the apparatus, which is thus placed on it as an insulated table.

The electromotive force of the battery, as compared with a Daniell's (gravity) battery, was found to be as 1·03 to 1*, its internal resistance 70 ohms per cell, and it evolved 0·214 cub. centim. (0·0131 cub. inches) mixed gas per minute when passed through a mixture of 1 volume of sulphuric acid and 8 volumes of water in a

* Compared with a Daniell's battery, in which the zinc is immersed in dilute sulphuric acid in a porous cell, its electromotive force is about 3 per cent. less than the Daniell.

voltameter having a resistance of 11 ohms. The striking-distance of 1080 elements between copper wire terminals, one turned to a point, the other to a flat surface, in air is $\frac{1}{2\frac{1}{6}\frac{1}{3}}$ inch (0.096 millim.) to $\frac{1}{2\frac{1}{5}\frac{1}{0}}$ inch (0.1 millim.). The greatest distance through which the battery-current would pass continuously *in vacuo* was 12 inches (30.48 centims.) between the terminals in a carbonic acid residual vacuum. This battery has been working since the early part of November 1874, with, practically, a constant electromotive force.

Besides 2000 more cells like those just described, we are putting together 2000 cells, with the chloride of silver in the form of rods, which are cast on the flattened silver wires, as in a battery described by De La Rue and Müller*, but in other respects similar to the battery above described, the glass tubes being, however, somewhat larger in diameter; the rods of chloride of silver are enclosed in tubes open at the top and bottom, and formed of vegetable parchment, the object of these vegetable-parchment cases being to prevent contact between the zinc and chloride-of-silver rods. The internal resistance of batteries so constructed is only from 2 to 3 ohms per cell, according to the distance of the zinc and chloride-of-silver rods, and they evolve from 3 to 4.5 cub. centims. (0.18 to 0.27 cub. inch) per minute, in a voltameter having a resistance of 11 ohms. Their action is remarkably constant.

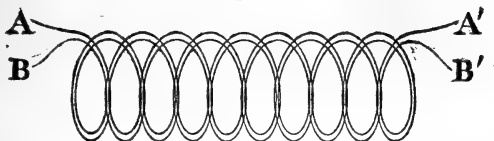
For the experiments detailed below, vacuum-tubes were generally used of about $1\frac{1}{2}$ to 2 inches (3.8 to 5 centims.) in diameter, and from 6 to 8 inches (15.24 to 20.32 centims.) long; also prolate spheroidal vessels 6 inches by 3 inches (15.24 by 7.62 centims.). The terminals are of various forms, and from 4 inches to 6 inches (10.16 to 15.24 centims.) apart, and made of aluminium and occasionally of magnesium and of palladium, the latter showing some curious phenomena with a hydrogen residual vacuum, which will be described in a future paper. A tube which has given the most striking results is 8 inches (20.32 centims.) long, and has a series of six aluminium rings varying in diameter from $\frac{3}{8}$ of an inch to about $1\frac{1}{4}$ of an inch (0.95 to 3.17 centims.), the thickness of the wire being about $\frac{1}{16}$ (0.16 centim.) of an inch; the rings are a little more than 1 inch (2.54 centims.) apart; and connecting wires of platinum pass through the tube from each ring and permit of the length and other conditions of the discharge being varied.

At times the terminals of the battery were placed in connexion with accumulators of different kinds—for instance, two spheres of 18 inches (45.72 centims.) in diameter, presenting each a superficies of 7.07 square feet (65.68 square decims.), and cylinders of paper covered with tinfoil, each having a surface of 16 square feet (148.64 square decims.); the globe and cylinders were in all cases carefully insulated. Other accumulators were composed of coils of two copper wires $\frac{1}{16}$ of an inch (0.16 centim.) in diameter, covered with gutta percha, in two folds, $\frac{1}{32}$ of an inch (0.08 centim.) thick. One coil contains two wires, A A' and B B' (fig. 1), coiled side by

* Journal of the Chem. Soc., 2nd series, vol. vi. p. 488; Comptes Rendus, 1886, p. 794.

side, each being 174 yards (159 metres) long, another with two wires each 350 yards (320 metres) long; of the latter we have two coils.

Fig. 1.

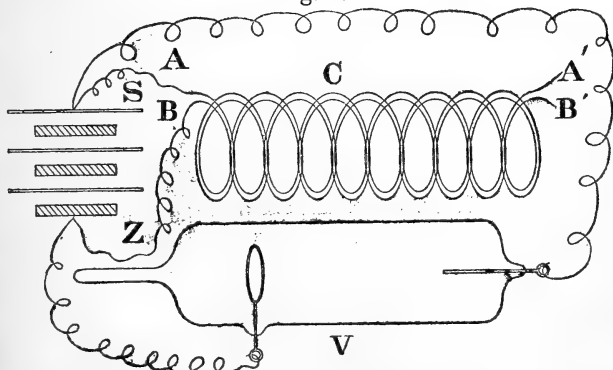


In addition to these accumulators we have several others formed of alternate plates of tinfoil and insulating material, such as paper saturated with paraffine, and also sheets of vulcanite. These are of various capacities and contain from 5 to several hundred square feet. The largest has a capacity of 47.5 microfarads; when it is discharged it gives a very bright short spark, accompanied by a loud snap; the charge deflagrates 8 inches (20.32 centims.) of platinum wire, .005 inch (0.127 millim.) in diameter, when it is caused to pass through it. Each accumulator gives different results; but for the present we shall confine ourselves to a description of the experiments made with the coil-accumulators.

When the terminals of the battery are connected with the wires of a vacuum-tube which permits of the passage of the current, the wires (especially that connected with the zinc end) become surrounded with a soft nebulous light, in which several concentric layers of different degrees of brilliancy are seen; in most cases there is either no indication of stratification, or only a feeble ill-defined tendency to stratification: the tubes selected for these experiments were those in which the stratification did not appear at all.

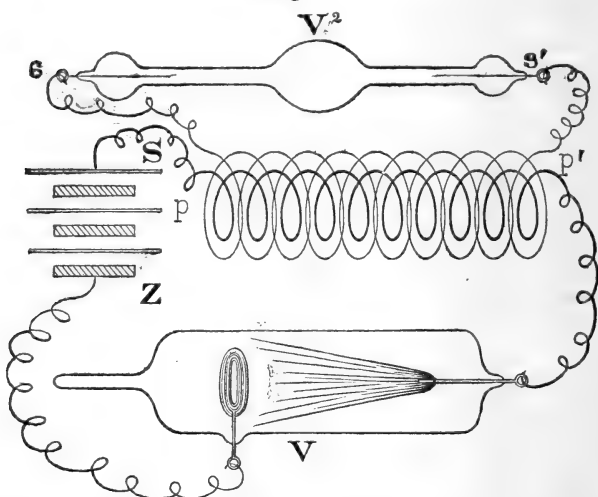
When the battery, already in connexion with the vacuum-tube, was also joined, as in fig. 2, on to one or more coil-condensers (coupled to introduce a greater length of wire) in the following manner, then immediately well-defined stratifications appeared in the vacuum-tube.

Fig. 2.



SZ represents the battery, V the vacuum-tube, C the coil-condenser; one terminal is connected with the end A of the wire A A', and the other terminal with the end B of the second wire B B'; connexions are also led to the wires of the vacuum-tube. The ends A' and B' are left free; and it is clear that the coil forms a sort of Leyden jar when thus used: an interval, however short it may be, must elapse in accumulating a charge which at intervals discharges itself and causes a *greater flow* in the vacuum-tube in addition to that which passes continuously. It may be stated that the capacity of the accumulator has to be carefully adjusted to prevent any cessation of the current, to avoid, in fact, a snapping discharge at distant intervals. The periodic overflows, so to speak, which increase the current from time to time, would seem to have a tendency to cause an interference of the current-waves, and to produce nodes of greater resistance in the medium, as evinced by the stratification which becomes apparent. To the eye no pulsation in the current is apparent; and in order to convince ourselves whether or not there was really any fluctuation in the current when the apparatus was thus coupled up with the battery, we made several experiments, and ultimately hit upon the following arrangement (fig. 3):—

Fig. 3.



The primary wire p p' of a small induction-coil, both with and without the iron core, was introduced into the circuit as well as the vacuum-tube V; to the secondary wire s s' of the induction-coil was connected a second vacuum-tube, V². Under these circumstances there was no change in the appearance of the discharge in V, in consequence of the introduction of the induction-coil, the terminals being still surrounded by the soft nebulous light before spoken of: no luminosity appeared in the second vacuum-tube V² in con-

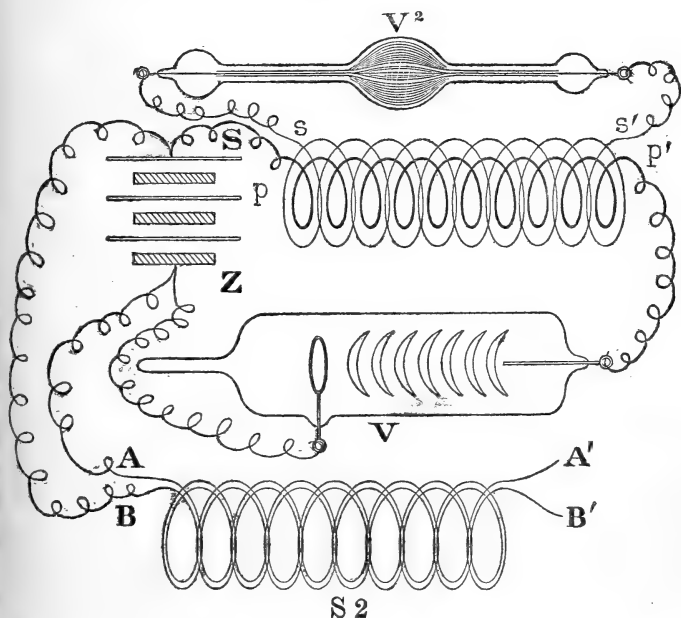
nexion with the secondary wire of the induction-coil, except on making and breaking the connexion with the battery. At other times there was evidently no fluctuation in the continuous discharge, no periodic increase or diminution of flow, and consequently no induced current in the secondary wire $s s'$ of the induction-coil.

In the second experiment wires were also led from the terminals of the battery (all other things remaining as before) to the coil-accumulator as in fig. 4; then immediately the discharge in V became stratified and the secondary vacuum-tube V^2 lighted up, clearly showing that under these circumstances a fluctuation in the discharge really occurs on the appearance of stratification.

The brilliancy of the discharge in V^2 (the induced current passes through complicated vacuum-tubes through which the primary current cannot pass) depends greatly on the quality and quantity of the discharge in the primary vacuum-tube V . Under some circumstances the secondary discharge is extremely feeble, and the illumination in V^2 barely visible; under others it is very brilliant.

Preparations are being made to render evident induced currents in the secondary wire of the coil too feeble to produce any illumination. Pending the further development of our investigation, we have ventured to give an account of our progress in elucidating some points in the theory of the vacuum-discharge, without any wish to ascribe to our results more weight than they deserve.

Fig. 4.



Batteries of this description may be had from Messrs. Tisley and Spiller, Brompton Road. Their cost, in large numbers, is about one shilling per cell, exclusive of the charge of chloride of silver, which costs about two shillings per cell. The latter, either in the form of powder or of rods cast upon flattened silver wire, may be obtained from Messrs. Johnson and Matthey, Hatton Garden. When the battery is exhausted the reduced silver may be readily reconverted into chloride, with scarcely any loss.

April 22.—John Evans, Esq., Vice-President in the Chair.

The following Papers were read:—

“On the Action of Heat on the Absorption-Spectra and Chemical Constitution of Saline Solutions.” By Walter Noel Hartley, F.C.S.

The effects of heat on absorption-spectra were recorded in the preliminary notice of this paper, published in the ‘Proceedings of the Royal Society’ for 1874 (vol. xxii. p. 241).

The contents of the present communication consist of:—1st, historical notes; 2nd, method of working; 3rd, the spectrum-measurements of different solutions; 4th, conclusions as to the effect of heat on coloured liquids, and the following deductions as to the constitution of salts when dissolved in water:—

I. When a simple metallic salt is dissolved in water, it is not decomposed in such a way that an oxide and an acid is produced, nor does a compound of the metallic oxide with the acid result.

II. When a metallic salt is dissolved in water to form a saturated solution, it does not necessarily attain its maximum state of hydration.

III. When a simple hydrated metallic salt is dissolved in water to form a saturated solution, the crystalline molecule remains chemically intact, except in the case of certain compounds which readily part with their water of crystallization, when dehydration takes place to form a molecule of greater stability; or, in other words, solution facilitates chemical change in this as in most other cases.

IV. When a simple salt assumes one or more definite states of hydration at different temperatures below 100° C., the hydrated compounds A and B will be successively produced in the liquid state if a saturated solution of the original salt be heated to 100° C.; or, in other words, the chemical constitution of the liquid is altered so that, as higher temperatures are attained, it becomes a solution of substance A or of substance B, at intermediate temperatures mixtures of these.

V. The action of heat on the violet hydrated compounds of chromium is not simply a dissociation of water-molecules or of acid from base, but a true decomposition, resulting in the production of a different class of salts with different generic properties.

Many new salts were prepared for this work, and others were examined with greater care than had previously been bestowed on

them; from these substances, indeed, the most important part of the results were derived.

“On Attraction and Repulsion resulting from Radiation.”—Part II. By William Crookes, F.R.S. &c.

This is the second part of a paper which the author sent to the Royal Society in August 1873. The author commences by describing improvements which he has made in the Sprengel pump, and in various accessories which are necessary when working at the highest rarefactions.

Continuing the description of apparatus, the author describes different new forms which enable the phenomena of repulsion by radiation to be observed and illustrated. A bulb 3 inches in diameter is blown at the end of a glass tube 18 inches long. In this bulb a fine glass stem, with a sphere or disk of pith &c. at each end, is suspended by means of a cocoon-fibre. The whole is attached to the Sprengel pump in such a way that it can be perfectly exhausted and then thermetically sealed. Besides pith, the terminals may be made of cork, ivory, metal, or other substance. During exhaustion several precautions have to be taken, which are fully entered into in the paper. To get the greatest delicacy in an apparatus of this kind, there is required large surface with a minimum of weight. An apparatus constructed with the proper precautions is so sensitive to heat, that a touch with the finger on a part of the globe near one extremity of the pith will drive the index round over 90° , whilst it follows a piece of ice as a needle follows a magnet. With a large bulb, very well exhausted and containing a suspended bar of pith, a somewhat striking effect is produced when a lighted candle is placed about 2 inches from the globe. The pith bar commences to oscillate to and fro, the swing gradually increasing in amplitude until the dead centre is passed over, when several complete revolutions are made. The torsion of the suspending fibre now offers resistance to the revolutions, and the bar commences to turn in the opposite direction. This movement is kept up with great energy and regularity as long as the candle burns.

The author discusses the action of ice, or a cold substance, on the suspended index. Cold being simply negative heat, it is not at first sight obvious how it can produce the opposite effect to heat. The author, however, explains this by the law of exchanges, and shows that attraction by a cold body is really repulsion by radiation falling on the opposite side. According to the same law, it is not difficult to foresee what will be the action of two bodies, each free to move, if they are brought near to one another in space, and if they differ in temperature either from each other or from the limiting walls of the space. The author gives four typical cases, with experiments, which prove his reasoning to be correct.

Experiments are described with the object of ascertaining whether the attraction by heat, which, commencing at the neutral point, increases with the density of the enclosed air, will be continued in

the same ratio if the apparatus is filled with air above the atmospheric pressure. This is found to be the case.

Various experiments are described with bulb-apparatus, in which the bulb is surrounded with a shell containing various adiathermous liquids and also with a shell of vacuum. In all cases radiation passed through, producing the normal action of attraction in air and repulsion in a vacuum.

The author next describes a form of apparatus by which measurable results are attainable. It consists of a long glass tube, with a wider piece at the end. In it is suspended a lump of magnesium by a very fine platinum wire, the distance between the point of suspension and the centre of gravity of the magnesium bob being 39·14 inches. Near the magnesium is a platinum spiral, capable of being ignited by a voltaic battery. Observations of the movement of the pendulum are made with a telescope with micrometer eyepiece. With this apparatus a large series of experiments are described, starting from air of normal density, and working at intermediate pressures up to the best attainable vacuum. The results are given in two tables.

With this apparatus it was found that a candle-flame brought within a few inches of the magnesium weight, or its image focused on the weight and alternately obscured and exposed by a piece of card at intervals of one second, will soon set the pendulum in vibration when the vacuum is very good. A ray of sunlight allowed to fall once on the pendulum will immediately set it swinging.

The form of apparatus is next described which the author has finally adopted, as combining the greatest delicacy with facility of obtaining accurate observations, and therefore of getting quantitative as well as qualitative results. It consists of a glass apparatus in the shape of an inverted T, and containing a horizontal glass beam suspended by a very fine glass thread. At the extremities of the beam are attached the substances to be experimented on, and at the centre of the beam is a small mirror from which a ray of light is reflected on to a graduated scale. The advantage which a glass thread possesses over a cocoon-fibre is that the index always comes accurately back to zero. In order to keep the luminous index at zero, except when experiments are being tried, extreme precautions must be taken to keep all extraneous radiation from acting on the torsion-balance. The whole apparatus is closely packed all round with a layer of cotton-wool about 6 inches thick; and outside this is arranged a double row of Winchester quart bottles filled with water, spaces only being left for the radiation to fall on the balance and for the index ray of light to get to the mirror.

However much the results may vary when the vacuum is imperfect, with an apparatus of this kind they always agree among themselves when the residual gas is reduced to the minimum possible; and it is of no consequence what this residual gas is. Thus, starting with the apparatus full of various vapours and gases, such as air, carbonic acid, water, iodine, hydrogen, ammonia, &c., at the

highest rarefaction, there is not found any difference in the results which can be traced to the residual gas. A hydrogen-vacuum appears the same as a water- or an iodine-vacuum.

With this apparatus the effect of exposing a torsion-balance to a continuous radiation is described, and the results are shown graphically. The effect of a short (11·3 seconds) exposure to radiation is next described, and the results are given in the form of a Table.

In another Table is given the results of experiments in which a constant source of radiation was allowed to act upon one end of the torsion-beam at a distance of 140 or 280 millims., various substances being interposed. The sensitiveness of this apparatus to heat-rays appears to be greater than that of an ordinary thermo-multiplier. Thus the obscure heat-rays from copper at 100° , passing through glass, produce a deflection on the scale of 3·25, whilst under the same circumstances no current is detected in the thermo-pile. The following substances are used as screens, and the deflections produced (when the source of radiation is magnesium wire, a standard candle, copper at 400° , and copper at 100°) are tabulated:—

Rock-salt, 20 millims. thick; rock-crystal, 42 millims. thick; dark smoky talc; plate glass of various thicknesses, both white and green; a glass cell containing 8 millims. of water; a plate of alum 5 millims. thick; calc-spar, 27 millims. thick; ammonio-sulphate of copper, opaque to rays below F; ditto, opaque to rays below G.

The author considers that these experiments show that the repulsion is not entirely due to the rays usually called heat, *i. e.* to the extreme and ultra red of the spectrum. Experiments have been tried with the electric and the solar spectrum formed with a quartz train, which prove the action to be also exerted by the luminous and ultra violet rays. Some numerical data have been obtained; but unfavourable weather has prevented many observations being made with the solar spectrum.

The barometric position of the neutral point dividing attraction from repulsion is next discussed. The position of this point varies with the density of the substance on which radiation falls, the ratio of its mass to its surface, its radiating and conducting-power for heat, the physical condition of its surface, the kind of gas filling the apparatus, the intensity of radiation, and the temperature of the surrounding atmosphere. The author is inclined to believe that the true action of radiation is repulsion at any pressure, and that the attraction observed when the rarefaction is below the neutral point is caused by some modifying circumstances connected with the surrounding gas, but not being of the nature of air-currents.

The neutral point for a thin surface of pith being low, and that for a moderately thick piece of platinum being high, it follows that at a rarefaction intermediate between these two points pith will be repelled, and that platinum will be attracted by the same beam of radiation. This is proved experimentally; and an apparatus showing simultaneous attraction and repulsion by the same ray of light is described and illustrated in the paper.

The paper concludes with a discussion of the various theories which have been adduced in explanation of these phenomena. The air-current and electrical theory are considered to have been abundantly disproved. The following experiment is given by the author to show that Prof. Osborne Reynolds's hypothesis of the movements due to evaporation and condensation at the surface will not account for all the facts of the case, and that therefore he has not hit upon the true explanation. A thick and strong bulb was blown at the end of a piece of very difficultly fusible green glass, specially made for steam-boiler gauges. In it was supported a thin bar of aluminium at the end of a long platinum wire. The upper end of the wire was passed through the top of the tube and well sealed in, for electrical purposes. The apparatus was sealed by fusion to the Sprengel pump, and exhaustion was kept going on for two days, until an induction-spark refused to pass across the vacuum. During this time the bulb and its contents were several times raised to a dull red heat. At the end of two days' exhaustion the tube was found to behave in the same manner as, but in a stronger degree than, it would in a less perfectly exhausted apparatus, viz. it was repelled by heat of low intensity and attracted by cold. A similar experiment was next tried, only water was placed in the bulb before exhaustion. The water was then boiled away *in vacuo*, and the exhaustion continued, with frequent heating of the apparatus to dull redness, for about 48 hours. At the end of this time the bar of aluminium was found to behave exactly the same as the one in the former experiment, being repelled by radiation.

It is impossible to conceive that in these experiments sufficient condensable gas or vapour was present to produce the effects Prof. Osborne Reynolds ascribes to it. After the repeated heating to redness at the highest attainable exhaustion, it is impossible that sufficient vapour or gas should condense on the movable index to be instantly driven off by the warmth of the finger with recoil enough to drive backwards a heavy piece of metal.

While objecting to the theories already advanced as not accounting for all the facts of the case, the author confesses that he is not as yet prepared with one to put in their place. He wishes to avoid giving any theory on the subject until a sufficient number of facts have been accumulated. The facts will then tell their own tale. The conditions under which they invariably occur will give the laws, and the theory will follow without much difficulty.

Supplement. Received April 20, 1875.

Since the experiments mentioned in the foregoing Abstract were concluded, the author has examined more fully the action of radiation on black and white surfaces. At the highest exhaustion heat appears to act almost equally on white and on lampblackened pith, repelling them in about the same degree.

The action of the luminous rays, however, is different. These

repel the black surface more energetically than they do the white surface. Taking advantage of this fact, the author has constructed an instrument which he calls a radiometer. This consists of four arms, suspended on a steel point resting on a cup, so that it is capable of revolving horizontally. To the extremity of each arm is fastened a thin disk of pith, lampblackened on one side, the black surfaces facing the same way. The whole is enclosed in a glass globe, which is then exhausted to the highest attainable point and hermetically sealed.

The author finds that this instrument revolves under the influence of radiation, the rapidity of revolution being in proportion to the intensity of the incident rays.

Several radiometers, of various constructions as regards details, but all depending on the above-named discovery, were exhibited by the author at the Soirée of the Royal Society on the 7th inst., and numerous experiments were shown with them. The following Table, which gives the result of some experiments tried with one of the first-made radiometers (and therefore not so sensitive as more recent instruments), is copied from a card which was distributed during the evening:—

“ Time required for One Revolution.

“ Source of radiation.		Time in seconds.
“ 1 candle, 20 inches off	182
“ 10 “	45
“ 5 “	11
2 candles, 5 “	5
4 “ 5 “	3
8 “ 5 “	1.6
1 candle, 5 “	behind green glass ..	40
“ 5 “	“ blue “ ..	38
“ 5 “	“ purple “ ..	28
“ 5 “	“ orange “ ..	26
“ 5 “	“ yellow “ ..	21
“ 5 “	“ light red “ ..	20
Diffused daylight, dull	2.3
“ “ bright	1.7
Full sunshine, 10 A.M.	0.3
“ “ 2 P.M.	0.25 ”

These experiments are not mentioned in the paper of which the above is an abstract, as it is intended to make the radiometer the subject of a future communication to the Society.

XXXI. *Intelligence and Miscellaneous Articles.*

ON THE ATTRACTION AND REPULSION EXERTED BY THE LUMINOUS AND THE CALORIFIC RAYS : BY DR. F. NEESEN*. AND ON CROOKES'S RADIOMETER : BY M. POGGENDORFF†.

MR. CROOKES'S remarkable experiments relating to the phenomena of attraction and repulsion which, he says, result from calorific and luminous radiations, could not fail to fix the attention of physicists ; yet the explanation which he gives of them must of necessity call forth numerous objections. It is, in fact, difficult to attribute to radiation the power to produce mechanical effects by acting through a vacuum—that is, with no other intermedium than the æther or extremely rarefied particles.

On the other hand, the high degree of rarefaction of the medium in which Mr. Crookes effected his experiments seems at first view to exclude the possibility of explaining the facts by merely the action of currents resulting from the inequalities of temperature of that medium, and especially the continuous rotation which he obtains in his radiometer.

Nevertheless the more recent researches of Dr. Neesen seem to prove that it is indeed currents of this nature that play the principal part in the phenomena in question.

M. Neesen used an apparatus which, it is true, does not permit the realization of so perfect a vacuum as that in which Mr. Crookes's pendulums or radiometer move, but which has the advantage of permitting the light or heat to be made to act in a perfectly determinate direction, and upon an equally determinate part of the suspended body.

This apparatus consists of a tin box, one of the sides of which has an opening fitted with a parallel-faced glass plate, giving passage to the calorific or luminous rays. The pendulum is fastened to the top of the box by means of a movable frame, so that it can be brought near to the glass plate or to the opposite side. It consists of a cocoon-thread to which is attached a piece of pasteboard or of wood covered with paper, carrying in its centre a small plane mirror, the deflections of which, observed at a distance with the cathetometer, measure the torsion of the thread. Finally, the light and heat are derived from a petroleum-lamp, the rays from which can be projected at will upon one of the sides of the pendulum, sometimes directly, sometimes concentrated by means of a lens.

Keeping at first the air contained in the box at the atmospheric pressure, the author satisfied himself that the radiation from the lamp really determines in it currents of transport. This appears clearly from the fact that the action of the lamp is different according as the pendulum is nearer to or further from the glass. By varying this distance it is ascertained that the deflection shown by the mirror is almost none when the pendulum is close to the

* Pogg. *Ann.* September 1875.

† Ibid. November 1875.

hinder side of the box, or, on the contrary, in the immediate vicinity of the glass; while it attains its maximum when the pendulum is nearly in the centre of the box, at an equal distance from its two opposite sides.

The thickness of the stratum of air between the pendulum and the sides of the box exerts, therefore, a great influence on the amplitude of the deflection, as it ought if this is due to the afflux of the air toward the warmer region of the interior atmosphere. By causing the rays to act alternately on the two sides of the pendulum, we can moreover ascertain that the deflection of the mirror certainly takes place always in the direction in which the colder air, situated behind the pendulum, goes to replace that which receives directly the radiations of the lamp.

But the heat from this has not only the effect of heating the air which it traverses, but the side of the pendulum which receives the rays is also not slow to be itself affected by its influence. Its temperature soon rises above that of the surrounding air; and a second set of currents are produced, going from the glass to the mirror and tending to turn it in the opposite direction to the former ones, which are directed from the posterior side of the box toward the glass plate.

The result is that the deflection due to the first currents diminishes, and finally changes its direction: this always takes place when the rays from the lamp fall directly on one of the sides of the pendulum to the right or the left of the mirror. But it is easy to prevent this diminution and inversion by concentrating the luminous rays by a lens on one of the sides of the pendulum, while the lamp heats directly the air situated on the other side.

In this case the currents of the second set evidently tend to turn the pendulum in the same direction as those produced by the direct heating of the air; and there ensues an increase instead of a diminution of the deflection.

The amplitude of the deflection of the mirror depends also notably on the angle at which it receives the rays—which is readily accounted for, since the intensity of the thermic action is itself in correspondence with the direction of those rays.

In fine, it is intelligible that the heating of the air must diminish with its density, while that of the pendulum must become more rapid and more intense in proportion as the air contained in the box is rarefied. Moreover Dr. Neesen has established that the deflection corresponding to the first set of currents diminishes in proportion as vacuum is produced in the apparatus, and is at last entirely supplanted by the opposite deflection resulting from the heating of the pendulum by the radiation from the lamp. Thus, according to him, is to be explained the fact observed by Mr. Crookes, from which he inferred that the action of heat on the pendulum is attractive in air, while it is repellent *in vacuo*.

Certainly Dr. Neesen's experiments tend to a rational explanation of the phenomena discovered by Mr. Crookes; nevertheless they cannot be considered altogether conclusive, since they were not performed in a medium reaching an extreme degree of rare-

faction, in which, perhaps, it would not be legitimate to admit the production of currents possessing sufficient energy to carry with them the relatively considerable mass of the pendulum.

It is, besides, very possible that these phenomena are actually due to currents as long as the rarefaction of the air contained in the apparatus does not exceed a certain degree; but it may also very well be that a totally different cause intervenes when the exhaustion is more complete. That cause, still obscure, may perhaps be the one suggested at first by Mr. Reynolds very shortly after Mr. Crookes's publication*.

Mr. Reynolds bases his interpretation on a consequence of the mechanical theory of gases. He remarks that the heating of gaseous particles which strike a surface hotter than themselves, and consequently the augmentation of their velocity, must be followed by a reaction in a direction opposite to that of the rays which act upon that surface, since the particles rebound with a greater velocity than that which they possessed before the impact. As, on the other hand, thermic radiation as well as the mobility of the particles must augment with the degree of rarefaction of the gas, this effect of reaction, he thinks, may still possess sufficient energy in the most highly rarefied media, where mere currents would have become too feeble.

Nevertheless M. Poggendorff, who has also occupied himself with this delicate subject, ranges himself rather on the side of the interpretation given by Dr. Neesen. It is true he does not regard the phenomenon as completely elucidated; but he is convinced that the motions, whether of the pendulum or of Crookes's radiometer, would certainly not take place in a perfect vacuum.

Using a radiometer constructed by Dr. Geissler, of Bonn, on the model of those employed by Mr. Crookes, M. Poggendorff has proved the perfect accuracy of the facts observed by the latter; he has even seen the vane of the radiometer put in motion merely by the influence of daylight from a sky covered with clouds. On the other hand, he has proved that the apparatus absolutely ceases to turn when the luminous rays which reach it have passed through a thickness of 20 millims. of water.

He notices also another fact, not less important. It is known that, in Mr. Crookes's experiments, the rotation of the vane does not take place when it is acted on by the obscure thermic radiation resulting from direct heating of the glass balloon which contains it. Now M. Poggendorff has obtained all the usual rotation effects by projecting upon his radiometer the rays from a wax taper after they have traversed a plate of black glass which is made use of in polarization-experiments, and which owes its opacity to the presence of a certain quantity of sulphur. Mr. Crookes, as well as Messrs. Tait and Dewar, had already proved that it is the same with the obscure radiation emanating from sulphiodide of carbon.

In brief, the phenomena discovered by Mr. Crookes are not yet completely explained; but it is probable that they are in great

* See Reynolds, "On the Forces caused by Evaporation from and Condensation at a Surface," *Proc. Roy. Soc.* June 1874, p. 406.

part due to the action of currents, and perhaps also to the reaction of gas-particles in highly rarefied media. In any case they are real facts, since they have been verified by several experimenters; and they appear to have led to the realization of an apparatus in some sort thermometric, endowed with extraordinary sensitiveness in regard to radiant heat.—*Bibliothèque Universelle, Archives des Sciences Phys. et Nat.* Jan. 15, 1876, pp. 84–88.

ON THE ACTION OF HEAT IN MAGNETIZATION. BY L. FAVÉ.

It has long been known that the magnetic state of a steel bar changes with the temperature. Coulomb, Kupffer, and other physicists have studied the very complex laws of the diminution of magnetic intensity; and the most marked consequence resulting from their experiments is, that by raising the temperature of steel to a certain point we cause it to lose definitively the magnetism it has received.

M. Jamin has recently demonstrated * that steel is capable of receiving considerable magnetization at a temperature at which it loses almost entirely that which it received when cold, while the magnetism of a bar magnetized when hot diminishes very rapidly, and in a very short time almost completely disappears.

We have investigated the variations of magnetic intensity by the method of Van Rees, which permits a speedy determination of the total quantity of free magnetism in a bar.

If a helix, formed of a few turns of copper wire wound round the bar, be slid quickly from the middle to a distance at which its influence is no longer sensible, the initial deflection of the galvanometer-needle, produced by the very feeble induced current resulting from the movement, measures sensibly the total quantity of free magnetism of the bar. We are assured that the earth's action is not sufficient to give an induced current that could disturb the result.

We operated on bars previously annealed, and cooled slowly, so that we could regard the changes of state as absolutely temporary, and the variations of temper did not complicate the phenomenon.

The bar to be examined is suspended horizontally to a copper rod, above a tube pierced with a series of apertures forming gas-jets close enough to heat the bar in a manner sensibly uniform throughout its length. This arrangement permits the magnetizing-coil to be introduced without the bar ceasing, even when inside, to be under the action of the source of heat—an important condition for following from its commencement the phenomenon of loss. The current from a pile of ten Bunsen elements is caused to pass round the bar; then the coil is withdrawn; the helix of copper wire, protected by a non-conducting envelope, is afterwards introduced as far as the middle of the bar, and then suddenly withdrawn; the initial deflection of the needle of a mirror-galvanometer produced by this last movement is measured. The temperature of the bar was given approximately by a thermoelectric couple.

* *Comptes Rendus*, December 22, 1873.

We thus observed that the quantity of residual magnetism slowly diminishes as the temperature of magnetization is increased; it is still considerable at incipient redness, but becomes insensible at a cherry-red heat.

The magnetism, which is rapidly lost if the bar is allowed to cool, is preserved at any temperature if the same calorific state is maintained—if not indefinitely, at least during a time so long that the very slow enfeeblement observed can be attributed to oscillations of temperature resulting from the difficulty of keeping a very high temperature perfectly constant. An augmentation or a diminution of temperature is accompanied by a loss of magnetism, which therefore does not depend on the temperature at which the bar was magnetized, but is a correlative phenomenon of the thermal state. The quantity of magnetism at a determinate temperature depends on the nature of the steel, the temperature of magnetization, and changes of state subsequently undergone.

When the bar is heating, the magnetism diminishes according to a law which depends on the composition of the steel, but in a pretty regular fashion; when it is cooling, the law of the loss of magnetism is very different from that of the loss of heat. The temperature diminishes, as we know, at first rapidly, then less and less quickly, and can be represented approximately by Newton's formula,

$$\theta = e^{at}.$$

The magnetic loss is, on the contrary, very slow at the commencement, then augments very rapidly, to resume afterwards a decreasing value. The annexed Table gives the quantity of magnetism, measured from minute to minute, in a bar magnetized at about 350° C. :—

Times	0	1	2	3	4	5	6	7	8	9	10	60
Quantities of magnetism } 29	29	28·8	28·7	27	19	12	8	6·5	5	4	1·5	
Temperatures	350°			200°				100°				12°

We see that it remains nearly constant during three minutes, then loses in four minutes two thirds of its value, lastly diminishes less and less quickly, and when the bar has arrived at the ordinary temperature has only $\frac{1}{20}$ of its initial value.

When we vary the temperature of magnetization, the time during which the loss remains very slight goes on increasing as the initial temperature is raised, although the cooling is more and more rapid. Besides, the time is independent of that during which the magnetism has been kept constant by maintaining the temperature.

When a bar which was magnetized when hot has been allowed to cool and is again heated, we observe that the quantity of magnetism increases and may attain to three times the value which it had preserved, remaining nevertheless below that possessed by it at the time of its magnetization. This is similar to M. Wiedmann's observation concerning the magnetic moment of bars magnetized at 100°.

But in following the quantity of magnetism during the heating

of the bar, it is seen to pass through a maximum, of which the temperature varies with the nature of the steel, but is always below the temperature of magnetization; if the heating is continued, the magnetism definitively vanishes. On leaving the temperature below that corresponding to the maximum, and letting the bar cool and then reheating it, the second maximum is found to be below the first, and so on. This property belongs solely to magnetism received at a high temperature and lost in consequence of cooling. When a bar which has received partial magnetization by contact with a magnet, or conserved after heating, is heated, the magnetism is seen to diminish more or less rapidly, but without there being any augmentation.

We have studied bars of divers compositions and different sections. Varying also the manner of heating, the quantities of magnetism conserved at the same temperatures have shown themselves to be different; but we have constantly observed these three phenomena :—

- (1) The conservation of the magnetism at any temperature when that temperature is maintained constant;
- (2) The diminution, at first slow, of the magnetism, becoming very rapid after a time variable with the temperature of magnetization;
- (3) The augmentation of the quantity of magnetism remaining after cooling when the magnet is reheated.

The operations were performed in the laboratory of the Sorbonne.—*Comptes Rendus de l'Académie des Sciences*, Jan. 24, 1876, vol. lxxxii. pp. 276–278.

ON THE PHENOMENA OF INDUCTION. BY M. MOUTON.

The experiments which have been made and the theories formed in regard to induction-phenomena have generally referred to an induced wire included in a circuit; the electricity set in motion passed from one extremity to the other, either through the wire of a galvanometer or a magnetizing spiral, or through the air under the complex form of the spark. But we may ask, into what relative electric condition does the phenomenon of induction bring the two extremities of the wire of an induction-coil when no metallic communication is established between them, and when they are kept at too great a distance for the spark to clear it?

For the purpose of solving this problem, I made use of a Thomson's dial electrometer of large size; the needle was maintained charged by its communication with one of the poles of an open pile; and the two pairs of dials were, at a suitable time, connected with the ends of the wire by the intermedium of a condenser with a stratum of air. If, limiting ourselves to the case of the rupture of the inducing current, we suppose a commutator putting, at the same time that it produces that rupture, the ends of the induced wire in communication with the dials of the electrometer, the following is what will be observed.

If the duration of the communication, commencing with the breaking of the current, exceeds a time which I have not tried to estimate rigorously, but which is not much less than $\cdot 001$ of a second, the condenser does not become charged, and the needle of the electrometer remains at rest.

If the duration of this communication is reduced to much less than $\frac{1}{20000}$ of a second, we find charges absolutely different according to the instant at which this rapid contact is produced; and we can thus study, from less than $\frac{1}{20000}$ to $\frac{1}{200000}$ of a second, the difference of potential presented by the extremities of the induced wire.

The following is then the succession of the phenomena which I have observed:—

Take the times for abscissæ, the origin being at the metallic rupture of the inducing current, and the differences of potential (or, what comes to the same thing, the deflections of the needle) for ordinates.

At the time zero the difference of potential is *nil*; it then increases regularly, and at about $\frac{1}{10000}$ of a second reaches a maximum which can be kept to a remarkable degree constant; it afterwards decreases regularly, again becoming *nil*. But the phenomenon does not end there. If the examination be continued, the difference of potential changes sign, the positive extremity becomes negative, and *vice versa*; there is a new maximum reached in the opposite direction, then a return to zero, then another change of sign, &c. I have counted in this way as many as 30 oscillations. The first oscillations have always seemed to me longer and more intense; they appear to tend rapidly towards isochronism. I beg, however, permission to reserve this question of the duration of the oscillations, which a special commutator, I hope, will soon enable me to determine.

In regard to the intensities the results are very clear. I will give an instance:—

The inducing current being supplied by a single Daniell element with water and sulphate of copper, the inducing bobbin having only one thickness of wire, and the induced bobbin 10,000 turns of wire $\frac{1}{2}$ millim. in diameter, the first and second maxima reached a difference of potential equivalent to 80 Daniell elements, betrayed by a deflection of 350 divisions by the needle of the electrometer, which I was obliged purposely to render less sensitive. The 20th maximum reached 160 divisions.

The oscillatory phenomenon was first described by M. Blaserna; it was measured more carefully, but still only with respect to its duration, by M. Bernstein,—both of these physicists sending the induced current for only a very brief period into a galvanometer.

I study the phenomenon in its entirety, from a purely static point of view, without disturbing it; and I get a constancy and intensity of effect which permit me to trace, point by point, the curve which represents it.—*Comptes Rendus de l'Académie des Sciences*, vol. lxxxii. pp. 84–86.

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XXXII. *On Waves.* By LORD RAYLEIGH, M.A., F.R.S.*

THE theory of waves in a uniform canal of rectangular section, in the case when the length of the wave is great in comparison with the depth of the canal, and when the maximum height of the wave is small in comparison with the same quantity, was given long ago by Lagrange, and is now well known. A wave of any form, subject to the above conditions, is propagated unchanged, and with the velocity which would be acquired by a heavy body in falling through half the depth of the canal. The velocity of propagation here referred to is of course relative to the undisturbed water. If we attribute to the water in the canal a velocity equal and opposite to that of the wave, the wave-form, having the same relative velocity as before, is now fixed in space, and the problem becomes one of *steady* motion. It is under this aspect that I propose at present to consider the question; and we will therefore suppose that water is flowing along a tube, whose section undergoes a temporary and gradual alteration in consequence of a change in the vertical dimension of the tube. The principal question will be how far the pressure at the upper surface can be made constant by a suitable adjustment of the velocity of flow to the force of gravity.

That the two causes which tend to produce variation of pressure at the upper surface act in opposition to each other is at once evident. If there were no gravity, the pressure would vary on account of the alteration in the velocity of the fluid. Since there must be the same total flow across all sections of

* Communicated by the Author.

the pipe, the fluid which approaches an enlargement must lose velocity, and the change of momentum involves an augmented pressure. On this account, therefore, there is an increased pressure at a place of enlargement and a diminished pressure at a contraction. On the other hand the effect of gravity is in the opposite direction, tending to produce a loss of pressure at the upper surface where that surface is high, and a gain of pressure where the surface is low. This effect of gravity is independent of the velocity; but the changes of pressure due to acceleration and retardation depend on the velocity of flow, and we can therefore readily understand that, with a certain definite velocity of flow, compensation may take place, at least approximately. When this happens, the condition of a free surface is satisfied, the constraint may be removed, and we are left with a stationary wave-form.

In the theory of long waves it is assumed that the length is so great in proportion to the depth of the water, that the velocity in a vertical direction can be neglected, and that the horizontal velocity is uniform across each section of the canal. This, it should be observed, is perfectly distinct from any supposition as to the height of the wave. If l be the undisturbed depth, and h the elevation of the water at any point of the wave, u_0 , u the velocities corresponding to l , $l+h$ respectively, we have, by the condition of continuity,

$$u = \frac{lu_0}{l+h},$$

so that

$$u_0^2 - u^2 = u_0^2 \frac{2lh + h^2}{(l+h)^2}.$$

By the principles of hydrodynamics, the increase of pressure due to retardation will be

$$\frac{\rho}{2} (u_0^2 - u^2) = \frac{\rho u_0^2}{2} \frac{2lh + h^2}{(l+h)^2}.$$

On the other hand, the loss of pressure due to height will be $g\rho h$; and therefore the total gain of pressure over the undisturbed parts is

$$\left\{ \frac{\rho u_0^2}{l} \frac{1 + \frac{h}{2l}}{\left(1 + \frac{h}{l}\right)^2} - g\rho \right\} h.$$

If now the ratio $h:l$ be very small, the coefficient of h becomes

$$\rho \left(\frac{u_0^2}{l} - g \right),$$

and we conclude that the condition of a free surface is satisfied provided $u_0^2 = gl$. This determines the rate of flow in order that a stationary wave may be possible, and gives of course at the same time the velocity of a wave in still water.

If we suppose the condition $u_0^2 = gl$ satisfied, the change of pressure is, to a second approximation,

$$\delta p = g\rho h \left\{ \frac{1 + \frac{h}{2l}}{\left(1 + \frac{h}{l}\right)^2} - 1 \right\} = -\frac{3}{2} \frac{g\rho h^2}{l},$$

which shows that the pressure is defective at all parts of the wave where h differs from zero. Unless, therefore, h^2 can be neglected, it is impossible to satisfy the condition of a free surface for a stationary long wave—which is the same as saying that it is impossible for a long wave of finite height to be propagated in still water without change of type. If, however, h be everywhere positive, a better result can be obtained with an increased value of u_0 ; and if h be everywhere negative, with a diminished value. We infer that a positive wave moves with a somewhat higher, and a negative wave with a somewhat lower velocity than that due to half the undisturbed depth.

Although a constant gravity is not adequate to compensate the changes of pressure due to acceleration and retardation in a long wave of finite height, it is evident that complete compensation is attainable if gravity be a function of height; and it is worth while to inquire what the law of force must be in order that long waves of unlimited height may travel with type unchanged. If f be the force at height h , the condition of constant pressure is

$$\frac{\rho}{2} u_0^2 \left\{ 1 - \frac{l^2}{(l+h)^2} \right\} = \rho \int_0^h f dh;$$

whence

$$f = -\frac{u_0^2}{2} \frac{d}{dh} \frac{l^2}{(l+h)^2} = u_0^2 \frac{l^2}{(l+h)^3},$$

which shows that the force must vary inversely as the cube of the distance from the bottom of the canal. Under this law the waves may be of any height, and they will be propagated unchanged with the velocity \sqrt{fl} , where f is the force at the *undisturbed level*.

The same line of thought may be applied to the case of a long wave in a canal whose section is uniform but otherwise arbitrary. Let A be the area of the section below the undis-

turbed level, b the breadth at that level. Then, as before,

$$(A + bh)u = Au_0$$

if h be small; whence

$$u_0^2 - u^2 = \frac{2bh}{A} u_0^2$$

Now by dynamics

$$u_0^2 - u^2 = 2gh$$

if the upper surface be free; and thus

$$u_0^2 = gA \div b,$$

which gives the velocity of propagation. In the case of a rectangular section we have the same result as before, since $A = bl$.

The energy of a long wave is half potential and half kinetic. If we suppose that initially the surface is displaced, but that the particles have no velocity, we shall evidently obtain (as in the case of sound) two equal waves travelling in opposite directions, whose total energies are equal, and together make up the potential energy of the original displacement. Now the elevation of the derived waves must be half of that of the original displacement, and accordingly the potential energies less in the ratio of 4 : 1. Since therefore the potential energy of each derived wave is one quarter, and the total energy one half of that of the original displacement, it follows that in the derived wave the potential and kinetic energies are equal.

We may now investigate the effect on a long wave of a gradual alteration in the breadth of the canal and the area of the section. The potential energy of the wave varies directly as the length, breadth, and square of the height; and, by what has been proved above, the same is true of the total energy. Now the length of the wave in various parts of the canal is obviously proportional to the velocity of propagation, viz. $\sqrt{\frac{A}{b}}$; and we may therefore write

$$E \propto \sqrt{\frac{A}{b}} \cdot (\text{height})^2 \cdot b.$$

But when the alteration in the canal is very gradual, there is no sensible reflection and the energy of the wave continues constant; so that

$$\text{height} \propto A^{-\frac{1}{4}} b^{-\frac{1}{4}}.$$

In the case of a rectangular section,

$$\text{height} \propto l^{-\frac{1}{4}} b^{-\frac{1}{4}}.$$

These results are due to Green, Kelland, and Airy*. The same method may be even more easily applied to the sound-wave moving in a pipe of gradually varying section.

The theory of long waves may be applied in many cases to ascertain the effect on a stream of a contraction or enlargement of its channel. If the section of the channel up to the natural level of the stream be altered from A_0 to A , the equation of continuity gives

$$(A + bh)u = A_0 u_0,$$

where b , the breadth at the surface of the water, is supposed not to vary with height. The condition of a free surface is

$$u_0^2 - u^2 = u_0^2 \left\{ 1 - \frac{A_0^2}{(A + bh)^2} \right\} = 2gh,$$

or

$$\frac{A_0^2}{(A + bh)^2} = 1 - \frac{2gh}{u_0^2},$$

which shows that h can never exceed the height due to the velocity u_0 , as is indeed otherwise obvious.

If the variations in A and b are small as well as gradual, and if we put $A = A_0 + \delta A$, we find

$$h = \frac{\delta A}{b} \div \frac{gA_0}{bu_0^2} - 1.$$

When the velocity u_0 is less than that of a free wave, $gA_0 > bu_0^2$, and h has the same sign as δA ; viz. a contraction of the channel produces a depression of the surface, and an enlargement an elevation. But if the velocity of the stream exceed that of a free wave, these effects are reversed, and an enlargement and contraction of the section entail respectively a depression and an elevation of the surface.

If the velocity of the stream is nearly the same as that of a free wave, a state of things is approached in which a wave can sustain itself in a stationary position without requiring a variation in the channel; and then the effects of such a variation are naturally much intensified.

We must not forget that these calculations proceed on the supposition that a steady motion is possible. It would appear that the motion thus obtained is unstable in the case where the velocity of the stream exceeds that of a free wave. If we suppose the upper surface to consist of a movable envelope, it would indeed be in equilibrium when disposed according to the law above investigated; but if a displacement be made and steady motion be conceived to be reestablished, the pressure of

* Stokes, Brit.-Assoc. Report on Hydrodynamics, 1846.

the fluid will be less than before if the displacement be downwards, but will be increased if the displacement be upwards; so that the forces brought to bear on the envelope are in both cases in the direction of displacement. The expression for the variation of pressure at the envelope is

$$\frac{\delta p}{\rho} = u_0^2 \frac{\delta A}{A_0} + \left(\frac{u_0^2 b}{A_0} - g \right) h.$$

The Solitary Wave.

This is the name given by Mr. Scott Russell to a peculiar wave described by him in the British-Association Report for 1844. Since its length is about six or eight times the depth of the canal, this wave is, to a rough approximation, included under the theory of long waves; but there are several circumstances observed by Mr. Russell which indicate that it has a character distinct from that of other long waves. Among these may be mentioned the very different behaviour of solitary waves according as they are positive or negative, viz. according as they consist of an elevation or a depression from the undisturbed level. In the former case the wave has a remarkable permanence, being propagated to great distances without much loss; but a negative wave is soon broken up and dissipated.

Airy, in his treatise on Tides and Waves, still probably the best authority on the subject, appears not to recognize any thing distinctive in the solitary wave. He says:—"We are not disposed to recognize this wave as deserving the epithets 'great' or 'primary,' . . . and we conceive that ever since it was known that the theory of shallow waves of great length was contained in the equation $\frac{d^2 X}{dt^2} = g\kappa \frac{d^2 X}{dx^2}$, . . . the theory of the solitary wave has been perfectly well known." And again, "Some experiments were made by Mr. Russell on what he calls a *negative* wave—that is, a wave which is in reality a progressive hollow or depression. But (we know not why) he appears not to have been satisfied with these experiments, and has omitted them in his abstract. All the theories of our IVth Section, without exception, apply to these as well as to *positive* waves, the sign of the coefficient only being changed."

On the other hand, Professor Stokes says*:—"It is the opinion of Mr. Russell that the solitary wave is a phenomenon *sui generis*, in no wise deriving its character from the circumstances of the generation of the wave. His experiments seem to ren-

* British-Association Report.

der this conclusion probable. Should it be correct, the analytical character of the solitary wave remains to be discovered."

The theory of the solitary wave has been considered by Earnshaw (*Camb. Trans.* vol. viii.), who, distrusting what he calls analytical approximations, bases his calculation on a supposed result of experiment, namely that the horizontal velocity is uniform over each section. This, as we have seen, is the fundamental assumption in the theory of long waves; but when the length of the wave is moderate, such a state of things is impossible in a frictionless fluid which has been once at rest; for it involves molecular rotation. In fact if there be a velocity-potential, the horizontal velocity u satisfies Laplace's equation

$$\frac{d^2u}{dx^2} + \frac{d^2u}{dy^2} = 0,$$

and therefore cannot be a function of x without being also a function of y . The motion investigated by Earnshaw has therefore molecular rotation; and the rotation remains constant for each particle; otherwise the equations of fluid motion would not be satisfied. This is the explanation of the difficulty with which Earnshaw meets,—that while the necessary conditions are satisfied in the wave itself, there is discontinuity in passing from the wave to the undisturbed water. The discontinuity arises from the fact that, as there is no rotation outside the wave, it is necessary to suppose finite rotations imparted to the particles as the wave reaches and leaves them. It is evident that, except in the case of very long waves, u must be treated as a function of y as well as of x .

In considering the theory of long waves (reduced to rest by imparting an opposite motion to the water), we saw that it was impossible to satisfy the condition of a free surface if the height of the wave were finite. It occurred to me to inquire whether there might not be compensation in certain cases between the variation of pressure at the upper surface due to a finiteness of height, and the variation due to a departure from the law of uniform horizontal velocity proper to very long waves. It was conceivable that the surface-condition in the case of a wave of given finite height might be better satisfied by a moderate than by a very great wave-length. In this way I have obtained what seems to be a perfectly satisfactory approximate theory of the solitary wave.

If u and v be the horizontal and vertical velocities in a stream moving in two dimensions without molecular rotation, and ϕ , ψ the potential and stream functions, we have

$$\left. \begin{aligned} u &= \frac{d\phi}{dx} = \frac{d\psi}{dy}, \\ v &= \frac{d\phi}{dy} = -\frac{d\psi}{dx}. \end{aligned} \right\} \dots \dots \dots (A)$$

Hence, if the bottom of the canal be taken for axis of x , we may take for u and v , since they satisfy Laplace's equation,

$$\left. \begin{aligned} u &= \cos\left(y \frac{d}{dx}\right) f(x) = f - \frac{y^2}{1.2} f'' + \frac{y^4}{1.2.3.4} f^{iv} - \&c., \\ -v &= \sin\left(y \frac{d}{dx}\right) f(x) = y f' - \frac{y^3}{1.2.3} f''' + \dots, \end{aligned} \right\} (B)$$

where $f(x)$ is the slowly variable value of u at the bottom when $y=0$, and accents indicate differentiation with respect to x . The corresponding expression for ψ is

$$\psi = yf - \frac{y^3}{1.2.3} f'' + \frac{y^5}{1.2.3.4.5} f^{iv} - \dots (C)$$

This equation applies to the upper boundary, if we understand by ψ the there constant value of the stream-function, and gives us a relation between the ordinate of the boundary and the function f .

If p be the pressure at the upper surface, we have

$$-2 \frac{p-C}{\rho} = 2gy + u^2 + v^2,$$

where C is some constant. We will write for brevity,

$$u^2 + v^2 = \varpi - 2gy; \dots \dots \dots (D)$$

and the object of the investigation is to examine how far it is possible to make ϖ constant by varying the form of y as a function of x . Since $u^2 + v^2 = (1 + y'^2)u^2$, our equation becomes

$$yu = \sqrt{\frac{\varpi y^2 - 2gy^3}{1 + y'^2}},$$

or, on substituting for u its value,

$$fy - \frac{y^3}{1.2} f'' + \frac{y^5}{1.2.3.4} f^{iv} - \&c. = \sqrt{\frac{\varpi y^2 - 2gy^3}{1 + y'^2}}.$$

Between this equation and (C), f may be eliminated by successive approximation; and we obtain as the relation between y and ϖ ,

$$\psi \left\{ 1 - \frac{y^3}{3} \left(\frac{1}{y}\right)'' - \frac{y^5}{45} \left(\frac{1}{y}\right)^{iv} - \dots \right\} = \sqrt{\frac{y^2 - 2gy^3}{1 + y'^2}}. \dots (E)$$

In this investigation y is regarded as a function of x , which

varies slowly, or (as we may put it) a function of ωx , where ω is a small quantity. If we agree to neglect the fourth power of ω , the third and following terms on the left-hand side of (E) may be omitted, and we obtain

$$\psi^2 \left\{ 1 + y'^2 - \frac{2}{3} y^3 \frac{d^2 y^{-1}}{dx^2} \right\} = \varpi y^2 - 2gy^3,$$

or

$$\psi^2 \left\{ 1 - \frac{1}{3} y'^2 + \frac{2}{3} y y'' \right\} = \varpi y^2 - 2gy^3, \quad . \quad . \quad (F)$$

by which the value of ϖ is determined approximately in terms of the form of the upper surface. If we suppose ϖ constant and integrate (F) on that hypothesis, we shall obtain a form of upper surface for which the pressure varies very slightly, provided of course that the solution so obtained satisfies the suppositions on which the differential equation (F) is founded.

To integrate (F) we may write it in the form

$$\psi^2 \left\{ 1 + \frac{4}{3} y^{\frac{3}{2}} \frac{d^2 y^{\frac{1}{2}}}{dx^2} \right\} = \varpi y^2 - 2gy^3,$$

or

$$\frac{d^2 y^{\frac{1}{2}}}{dx^2} = \frac{3}{4\psi^2} (\varpi y^{\frac{1}{2}} - 2gy^{\frac{3}{2}} - \psi^2 y^{-\frac{3}{2}}),$$

which becomes a complete differential when multiplied by $2 \frac{dy^{\frac{1}{2}}}{dx} dx$. Thus we find

$$\frac{1}{3} y'^2 = Cy + \frac{\varpi y^2 - gy^3}{\psi^2} + 1,$$

C being the constant of integration. Suppose now that in the undisturbed parts of the canal the depth is l and the velocity u_0 . Then

$$\varpi = u_0^2 + 2gl,$$

and

$$\psi = \int_0^l u_0 dy = u_0 l.$$

Substituting these, we get

$$\frac{y'^2}{3} = 1 + Cy + \frac{u_0^2 + 2gl}{u_0^2 l^2} y^2 - \frac{g}{u_0^2 l^2} y^3. \quad . \quad . \quad (G)$$

In this equation g and l are given, while u_0 and C are at our disposal; and thus the cubic expression on the right may be made to vanish for $y=l$ and $y=l'$, where l' is the distance between the summit of the wave and the bottom of the canal. If

The shortest wave-length is when $\beta=l$; and then

$$2x:l=5.96.$$

$$\text{If } \beta=\frac{l}{3},$$

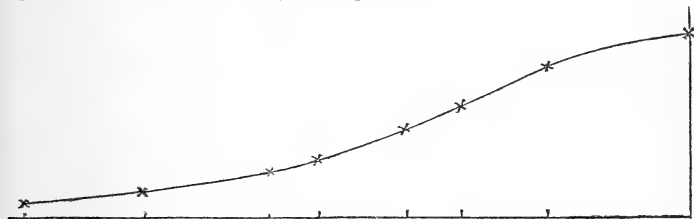
$$2x:l=8.4.$$

$$\text{If } \beta=\frac{l}{8},$$

$$2x:l=12.6.$$

These results are in agreement with Russell's observations.

The form of the wave as determined by (J) is shown in the figure, half the wave only being drawn:—



The velocity of propagation is given by (H), which is Scott Russell's formula exactly. In words, the velocity of the wave is that due to half the greatest depth of the water.

Another of Russell's observations is now readily accounted for:—"It was always found that the wave broke when its elevation above the general level became equal, or nearly so, to the greatest depth. The application of mathematics to this circumstance is so difficult, that we confine ourselves to the mention of the observed fact"*. When the wave is treated as stationary, it is evident from dynamics that its height can never exceed that due to the velocity of the stream in the undisturbed parts; that is, $l'-l$ is less than $\frac{u_0^2}{2g}$. But $u_0^2=g'l$, and therefore $l'-l$ is less than $\frac{1}{2}l'$, or $l'-l$ is less than l . When the wave is on the point of breaking, the water at the crest is moving with the velocity of the wave.

Periodic Waves in Deep Water.

The best known theory of these waves is that of Gerstner, Rankine, and Froude, in which the profile is trochoidal. The motion of each particle of the fluid is in a circle, which is described with uniform velocity. If h, κ be the coordinates of the centre of one of these circles, measured horizontally and downwards respectively, the position of the particle at time t

* Airy, 'Tides and Waves,' art. 401.

is given by

$$\left. \begin{aligned} \xi &= h + R e^{-\frac{\kappa}{R}} \sin \left(at + \frac{h}{R} \right), \\ \eta &= \kappa + R e^{-\frac{\kappa}{R}} \cos \left(at + \frac{h}{R} \right). \end{aligned} \right\}$$

It is not difficult to show that the motion represented by these equations satisfies the condition of continuity, and is consistent with the principles of fluid mechanics; but it involves a molecular rotation, whose amount is

$$a e^{-\frac{2\kappa}{R}} \div 1 - e^{-\frac{2\kappa}{R}}.$$

This molecular rotation, being constant for each particle, is not inconsistent with the properties of frictionless fluid when the motion is once set up; but it is known that a motion of this kind could not be generated from rest in such fluid by any natural force. We proceed to consider the theory of periodic waves in deep water when there is no molecular rotation.

As in the case of long waves, the problem may be reduced to one of steady motion by attributing to the water a velocity equal and opposite to that of the waves. If x be measured horizontally and y downwards from the surface, the conditions of continuity and of freedom from rotation are satisfied by

$$\left. \begin{aligned} \phi &= cx + \alpha e^{-\kappa y} \sin \kappa x, \\ \psi &= cy - \alpha e^{-\kappa y} \cos \kappa x; \end{aligned} \right\} \quad . \quad . \quad . \quad (A)$$

where ϕ and ψ are the equipotential and stream functions, c the velocity at a great depth, α a constant depending on the amplitude of the waves, and $\kappa = 2\pi \div \lambda$, λ being the wavelength or distance from crest to crest. The motion represented by (A) passes into a uniform horizontal flow at a great depth; and we have only to inquire how far the surface-condition of constant pressure can be satisfied.

If U be the resultant velocity at any point,

$$\begin{aligned} U^2 &= \left(\frac{d\phi}{dx} \right)^2 + \left(\frac{d\phi}{dy} \right)^2 = c^2 + 2c\kappa\alpha e^{-\kappa y} \cos \kappa x + \kappa^2 \alpha^2 e^{-2\kappa y} \\ &= c^2 + 2c\kappa(cy - \psi) + \kappa^2 \alpha^2 e^{-2\kappa y}, \end{aligned}$$

and therefore

$$\frac{U^2}{\rho} = \text{const.} + (g - \kappa c^2)y + c\kappa\psi - \frac{1}{2}\kappa^2 \alpha^2 e^{-2\kappa y}. \quad . \quad . \quad (B)$$

Hence, when ψ is constant and α is so small that α^2 can be

neglected, p will also be constant, provided that

$$c = \sqrt{\frac{g}{\kappa}} = \sqrt{\frac{g\lambda}{2\pi}}. \quad . \quad . \quad . \quad . \quad . \quad (C)$$

If c has this value, the surface-condition is satisfied approximately, and (A) may be understood to represent a train of free periodic stationary waves, or, if the motion relatively to deep water be considered, a train of periodic waves advancing without change of type and with a uniform velocity c .

The profile of the wave is determined by the second of equations (A), in which ψ is made constant. By successive approximation we may deduce the value of y in terms of x . If ψ be taken so that the mean value of y is zero, we get

$$y = \frac{\alpha}{c} \left(1 + \frac{5}{8} \frac{\kappa^2 \alpha^2}{c^2} \right) \cos \kappa x - \frac{\kappa \alpha^2}{2c^2} \cos 2\kappa x + \frac{3}{8} \frac{\kappa^2 \alpha^3}{c^3} \cos 3\kappa x,$$

which is correct as far as α^3 . Let

$$\frac{\alpha}{c} \left(1 + \frac{5}{8} \frac{\kappa^2 \alpha^2}{c^2} \right) = a;$$

then

$$\left. \begin{aligned} \phi &= cx + ca \left(1 - \frac{5}{8} \kappa^2 a^2 \right) e^{-\kappa y} \sin \kappa x, \\ \psi &= cy - ca \left(1 - \frac{5}{8} \kappa^2 a^2 \right) e^{-\kappa y} \cos \kappa x; \end{aligned} \right\} \quad . \quad . \quad (D)$$

and for the equation of the surface,

$$y = a \cos \kappa x - \frac{\kappa a^2}{2} \cos 2\kappa x + \frac{3}{8} \kappa^2 a^3 \cos 3\kappa x. \quad . \quad . \quad (E)$$

From (B) we may obtain a closer approximation to the value of c . Expanding the exponential, we have (approximately)

$$\frac{p}{\rho} = \text{const.} + (g - \kappa c^2 + \kappa^3 \alpha^2) y + \dots;$$

so that

$$c^2 = \frac{g}{\kappa} + \kappa^2 \alpha^2 = \frac{g}{\kappa} + \kappa^2 a^2 c^2,$$

or

$$c^2 = \frac{g}{\kappa} (1 + \kappa^2 a^2), \quad . \quad . \quad . \quad . \quad . \quad (F)$$

where $\kappa = 2\pi \div \lambda$.

Formulae (E) and (F) are given by Professor Stokes in a memoir published in the Cambridge Philosophical Transactions, vol. viii.

So long as the depth is everywhere sufficiently great in comparison with the length of the waves, uniformity of depth is

immaterial. For waves in water of constant finite depth l , the expression for ψ is

$$\psi = cy - \alpha \cos \kappa x \{e^{-\kappa(y-l)} - e^{\kappa(y-l)}\},$$

and the velocity c is determined by

$$c^2 = \frac{g}{\kappa} \cdot \frac{e^{\kappa l} - e^{-\kappa l}}{e^{\kappa l} + e^{-\kappa l}} \quad . \quad . \quad . \quad . \quad (G)$$

which passes into (C) when l is considerable in comparison with λ . When l is small, we get from (G)

$$c^2 = \frac{g}{\kappa} \cdot \frac{2\kappa l}{2} = gl,$$

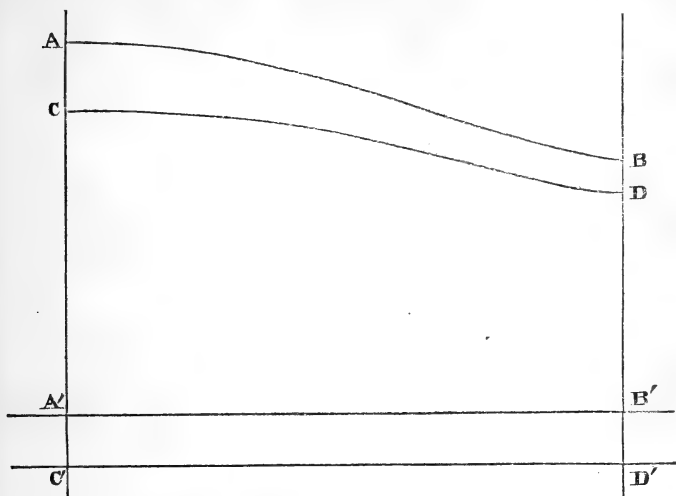
which is the formula proper for long waves. When obtained thus, it is applicable in the first instance only to waves of a particular type; but the fact that it is independent of κ or λ would lead us to the conclusion that the same formula would apply to a long wave of any type.

In one respect the theory of irrotational waves may be considered inferior to that of Rankine, which last is *exact*, in the sense that it is independent of any supposition as to the smallness of the waves. So far as I am aware, writers on this subject appear to think that it is only a question of mathematics to determine the form of irrotational waves of finite amplitude to any degree of approximation. But it seems to me by no means certain that any such type exists, capable of propagating itself unchanged with uniform velocity. I see no reason why the possibility of such waves in deep water should be taken for granted, when we know that in shallow water waves of finite height cannot be propagated without undergoing a gradual alteration of type.

One of the most interesting results of Professor Stokes's theory is the existence of a slow translation of the water near the surface in the direction of the wave. I propose to show that this superficial motion is an immediate consequence of the absence of molecular rotation, and that it is independent of the condition of constant pressure at the bounding surface.

Let AB be the surface from crest to hollow, and CD a neighbouring stream-line. Draw $A'B'$, $C'D'$, two stream-lines at such a depth that the steady motion of the fluid is uniform, and so as to include a total stream equal to that which flows between AB and CD . Then we have to show that a particle at A will take longer to reach B , than a particle at A'

takes to arrive at B'. Now if σ denotes the small breadth of the tube AD at any point, and v the velocity, the total stream



is σv and is constant. Denoting it by K , we have

$$v = K \div \sigma.$$

The time t occupied by a particle in removing from A to B is therefore

$$t = \int \frac{ds}{v} = \int \frac{\sigma ds}{K} = \text{area AD} \div K.$$

And if t' represents the time between A' and B',

$$t' = \text{area A'D'} \div K,$$

K being the same in both cases, since the total streams are by supposition equal. Thus

$$t : t' = \text{area AD} : \text{area A'D'},$$

and it remains to prove that AD is greater than A'D'.

If we draw equipotential lines in such a manner that the small spaces cut off between them and AB, CD are squares, then we know that the same series of equipotential lines will divide the space between A'B', C'D', into small squares also. Now if a line be divided into a given number of parts, the sum of the squares of the parts will be a minimum when the parts are all equal. Hence the space AD is greater than if the squares described on the parts of AB were all equal, and therefore *à fortiori* greater than the space A'D', which consists of

the sum of the squares of the same number of equal parts of a shorter line.

It follows that when a particle starting from A' has arrived at B', another particle starting at the same moment from A will fall short of B. Thus in a progressive wave the water near the surface has on the whole a motion of translation in the direction in which the waves advance.

Oscillations in Cylindrical Vessels.

If liquid contained in a cylindrical vessel of any section, whose generating lines are vertical and whose depth is uniform, be disturbed from the position of equilibrium, oscillations will ensue in consequence of the tendency of the fluid to recover its horizontal boundary.

Let us consider in the first place the small vibrations in two dimensions of a compressible fluid such as air when contained within a cylindrical rigid boundary. If x and y be the rectangular coordinates of any point, and ϕ the velocity potential, it is known that ϕ will satisfy over the whole area

$$\frac{d^2\phi}{dt^2} = a^2 \left(\frac{d^2\phi}{dx^2} + \frac{d^2\phi}{dy^2} \right), \quad (A)$$

a being the velocity of sound; while round the contour

$$\frac{d\phi}{dn} = 0, \quad (B)$$

where $\frac{d\phi}{dn}$ denotes the rate of variation of ϕ in a normal direction.

Whatever the motion of the air may be, it can be analyzed into components of the harmonic type. Suppose that for one of these ϕ varies as $\cos \kappa at$; then, from (A),

$$\frac{d^2\phi}{dx^2} + \frac{d^2\phi}{dy^2} + \kappa^2\phi = 0 \quad (C)$$

is an equation which ϕ must satisfy for the component vibration in question. The equations (C) and (B) can only be satisfied with certain definite values of κ ; and the functions ϕ corresponding to these values are proportional to what may be called the *normal functions* of the air-system. We may denote these functions by u_κ . Any function arbitrary over the area can be expanded in a series of the functions u^* .

Returning to the liquid-problem, we see that the elevation h of the surface at any point above the undisturbed position

* See on this subject several papers by the author, especially "General Theorems relating to Vibrations," Math. Society, Proceedings, vol. iv. No. 63, and Phil. Mag. December 1873.

may be expressed by the series

$$h = \sum \alpha_{\kappa} u_{\kappa}(xy), \quad . \quad . \quad . \quad . \quad . \quad (D)$$

the quantities α being constants with respect to space, but dependent upon time. The potential energy of the displacement, calculated on the hypothesis of a constant pressure on the surface, will clearly be

$$\left. \begin{aligned} V &= g\rho \iint \cdot \int_0^h dz \cdot dx dy = \frac{g\rho}{2} \iint h^2 dx dy \\ &= \frac{g\rho}{2} \iint (\sum \alpha_{\kappa} u_{\kappa})^2 dx dy = \frac{g\rho}{2} \sum \alpha_{\kappa}^2 \iint u_{\kappa}^2 dx dy \end{aligned} \right\}, \quad (E)$$

by the conjugate property of the functions u . This is the potential energy.

The motion of the fluid throughout the interior depends, according to a known theorem, only upon the motion of the surface; and the surface normal velocity

$$-\frac{d\phi}{dz} = \dot{h} = \sum \dot{\alpha}_{\kappa} u_{\kappa}.$$

If l be the depth, the complete value of ϕ is given by

$$-\phi = \sum \frac{e^{\kappa(z-l)} + e^{-\kappa(z-l)}}{\kappa(e^{-\kappa l} - e^{\kappa l})} \dot{\alpha}_{\kappa} u_{\kappa}(xy). \quad . \quad . \quad . \quad (F)$$

For, in the first place, this value of ϕ satisfies Laplace's equation, inasmuch as each term u_{κ} satisfies the equation

$$\frac{d^2 u}{dx^2} + \frac{d^2 u}{dy^2} + \kappa^2 u = 0;$$

Secondly, (F) satisfies the condition imposed by the rigid cylindrical boundary, since $\frac{du_{\kappa}}{dn} = 0$;

Thirdly, (F) makes $\frac{d\phi}{dz} = 0$ when $z = l$;

And fourthly, when $z = 0$, $-\frac{d\phi}{dz} = \dot{h}$.

The kinetic energy T may now be readily calculated:

$$\begin{aligned} T &= \frac{\rho}{2} \iiint \left\{ \left(\frac{d\phi}{dx} \right)^2 + \left(\frac{d\phi}{dy} \right)^2 + \left(\frac{d\phi}{dz} \right)^2 \right\} dx dy dz \\ &= \frac{\rho}{2} \iint \frac{d\phi}{dn} dS, \end{aligned}$$

by Green's theorem, dS denoting an element of the surface bounding the mass, and $\frac{d\phi}{dn}$ the rate of variation of ϕ in a normal direction outwards. The surface S consists of three parts—the bottom of the vessel, the cylindrical side of the vessel, and the upper surface of the fluid. Over the first two of these, $\frac{d\phi}{dn} = 0$, and thus

$$T = -\frac{\rho}{2} \iint \left(\phi \frac{d\phi}{dz} \right)_{z=0} dx dy.$$

Now when $z=0$,

$$\phi = \sum \frac{e^{\kappa l} + e^{-\kappa l}}{e^{\kappa l} - e^{-\kappa l}} \frac{\dot{\alpha}_\kappa}{\kappa} u_\kappa,$$

$$\frac{d\phi}{dz} = -\sum \dot{\alpha}_\kappa u_\kappa,$$

so that

$$T = \frac{\rho}{2} \sum \frac{\dot{\alpha}_\kappa^2}{\kappa} \frac{e^{\kappa l} + e^{-\kappa l}}{e^{\kappa l} - e^{-\kappa l}} \iint u_\kappa^2 dx dy, \quad \dots \quad (G)$$

the product of any two functions $u_\kappa, u_{\kappa'}$ vanishing when integrated over the area.

We have now to calculate the work done by impressed forces corresponding to the displacement represented by $\delta \alpha_\kappa$. It must be remembered that these forces are limited to such as have a potential. Let δp denote the variable part of the pressure at the surface, supposed to remain in its position of rest, whether applied directly or due to impressed body-forces, then

$$\text{work done on system} = - \iint \delta p \delta h dx dy.$$

If δp be expanded in the series,

$$\delta p = \sum \beta_\kappa u_\kappa(xy),$$

$$\begin{aligned} \text{work} &= - \iint \sum \beta_\kappa u_\kappa \cdot \sum \delta \alpha_\kappa u_\kappa \cdot dx dy \\ &= - \sum \beta_\kappa \delta \alpha_\kappa \iint u_\kappa^2 dx dy. \end{aligned}$$

We can now form the equations of motion in terms of the generalized coordinates α_κ . By Lagrange's method,

$$\frac{\ddot{\alpha}_\kappa}{\kappa} \frac{e^{\kappa l} + e^{-\kappa l}}{e^{\kappa l} - e^{-\kappa l}} + g \alpha_\kappa = - \frac{\beta_\kappa}{\rho} \quad \dots \quad (H)$$

is the equation determining the variation of the coordinate α_κ , where

$$\beta_\kappa = \iint \delta p u_\kappa dx dy \div \iint u_\kappa^2 dx dy.$$

When the oscillations are free, $\beta_\kappa = 0$. If the period be τ_κ , and

the corresponding period for the air-vibration τ'_κ ,

$$\left. \begin{aligned} \tau_\kappa &= 2\pi \div \sqrt{g\kappa \frac{e^{\kappa l} - e^{-\kappa l}}{e^{\kappa l} + e^{-\kappa l}}}, \\ \tau'_\kappa &= 2\pi \div \kappa a. \end{aligned} \right\} \dots \dots (I)$$

If λ' be the wave-length of plane aerial vibrations having the period τ'_κ ,

$$\kappa = 2\pi \div \lambda'.$$

If κl be very small, the ratio of periodic times is

$$\tau_\kappa : \tau'_\kappa = a : \sqrt{gl}, \dots \dots (J)$$

and is independent of κ . Hence the two problems of the vibrations of air and liquid are mathematically analogous whatever the initial circumstances may be; so that if the condensation in the first follows the same law initially as the elevation in the second, the correspondence will be preserved throughout the subsequent motion, if $a^2 = gl$. The initial circumstances, however, must be such as not to give prominence to the higher components, for which κl would no longer be small.

When κl is not negligible, we learn from formula (I) that the period increases with l until κl is moderately great, when it becomes sensibly

$$\tau_\kappa = 2\pi \div \sqrt{g\kappa}. \dots \dots (K)$$

In any case the period is independent of the density of the liquid.

Some careful observations on liquid vibrations have been recently made by Professor Guthrie*, with which it may be interesting to compare the results of theory. Professor Guthrie used troughs whose horizontal section was rectangular and circular. We will take the rectangular section first.

Confining ourselves to those modes of vibration which depend on only one horizontal coordinate, we may take for the normal functions

$$u = \cos \frac{n\pi x}{L},$$

L being the length of the trough, n integral, and x being measured from one end. The corresponding value of κ is $\frac{n\pi}{L}$.

Hence, from (I), the length of the simple equivalent pendulum is

$$\frac{L}{n\pi} \frac{e^{\frac{n\pi l}{L}} + e^{-\frac{n\pi l}{L}}}{e^{\frac{n\pi l}{L}} - e^{-\frac{n\pi l}{L}}}. \dots \dots (L)$$

* Phil. Mag. October and November 1875.

When $\frac{n\pi l}{L}$ is considerable, (L) becomes

$$\frac{L}{n\pi}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (M)$$

or, for a closer approximation,

$$\frac{L}{n\pi} \left(1 + 2e^{-\frac{2n\pi l}{L}}\right). \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (N)$$

Formula (M) was found by Professor Guthrie to agree with observation when $n=1$ or 2 . The periods in the two cases are in the ratio $1 : \sqrt{2}$, if the depth be sufficient.

If the depth bear a constant ratio to the length, (L) or (I) shows that the period is directly proportional to the square root of the linear dimension; and the same law will obtain when the depth is great, whatever the absolute value may be.

If $n=1$, the points of constant elevation occur when $x = \frac{L}{2}$ (that is, in the middle of the length); and if $n=2$, when $x = \frac{L}{4}$ or $\frac{3L}{4}$. The maximum elevations (or depressions) are equal.

These results take into account inertia and gravity only. From some expressions in his paper Professor Guthrie would appear to attribute the effect of shallowness in increasing the period to friction. No doubt friction must act in this direction; but its immediate effect is on the amplitude, and not on the period. In all ordinary cases the action of insufficient depth may be sufficiently accounted for by the increase of the effective inertia due to the contraction of the channels along which the liquid flows, in the same way as the pitch of an organ-pipe is lowered by an obstruction at the mouth. In such vessels as those used by Professor Guthrie it may be doubted whether friction and capillarity have any sensible influence on the periodic time.

The theory for the circular trough depends on the class of functions named after Bessel, which are an extreme case of Laplace's spherical functions. For the symmetrical vibrations we have

$$u = J_0(\kappa r), \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (O)$$

being the radius vector; and if R be the radius of the vessel, κ is a root of

$$J'_0(\kappa R) = 0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (P)$$

If $x = \kappa R$, the values of x satisfying (P) are 3.832, 7.015, 10.174, &c., of which only the first belongs to the cases experi-

mented on by Professor Guthrie. The approximate formula for the length of the simple equivalent pendulum corresponding to (N) is

$$\frac{R}{3.832} (1 + 2e^{-7.7 \frac{l}{R}}), \quad (Q)$$

or, when l is considerable,

$$R \div 3.832 \text{ simply.} \quad (R)$$

Professor Guthrie compares his observations with a pendulum of length $R \div 4$, and finds a fair agreement, which, however, would be improved by the substitution of the theoretical formula (R).

According to (O) the place of zero elevation and depression occurs when

$$r = \frac{2.405}{3.832} R = .6277 R.$$

According to observation,

$$r = \frac{2}{3} R = .6667 R.$$

From the Tables of Bessel's functions it appears that the amplitude at the edge of the vessel is .403 of that at the centre. Professor Guthrie makes this .5.

For the next set of vibrations in a circular dish u is of the form

$$u = \sin \theta J_1(\kappa r),$$

where the admissible values of κR are 1.841, 5.332, 8.536, &c. Hence for the gravest of this group the length of the equivalent pendulum is

$$R \div 1.841. \quad (S)$$

In this group of modes the elevation vanishes at all points along a certain diameter ($\theta = 0$).

In the third group we have

$$u = \sin 2\theta J_2(\kappa r),$$

and the admissible values of κR are 3.054, 6.705, 9.965, &c. For the gravest of these the length of the equivalent pendulum is

$$R \div 3.054, \quad (T)$$

if the depth be sufficient. The elevation vanishes along two perpendicular diameters ($\theta = 0, \theta = \frac{\pi}{2}$).

In the fourth group there would be three diameters for which $u = 0$; and the length of the pendulum isochronous with the gravest mode will be

$$R \div 4.201. \quad (U)$$

The frequencies of vibration in the three gravest modes, being inversely as the square root of the corresponding pendulum-lengths, are in the ratio

$$1 : 1.29 : 1.44.$$

Professor Guthrie's observations give for the value of these ratios

$$1 : 1.31 : 1.48.$$

Possibly too low a frequency is attributed to the gravest vibration from the effect of insufficient depth.

When the complete theory of the free vibrations of any system is thoroughly known, it is in general easy to investigate the effect of periodic forces. If u_1, u_2 , &c. are the normal functions, and $\frac{2\pi}{n_1}, \frac{2\pi}{n_2}$, &c. the periods of the corresponding

free vibrations, the effect of forces whose period is $\frac{2\pi}{p}$ can be expressed in terms of the effect produced by similar forces of infinite period, which last can be calculated statically. Thus, if the solution of the problem according to the equilibrium theory is

$$A_1 u_1 \cos pt + A_2 u_2 \cos pt + \dots,$$

the true solution as modified by the inertia of the system will be

$$\frac{A_1 n_1^2}{n_1^2 - p^2} u_1 \cos pt + \frac{A_2 n_2^2}{n_2^2 - p^2} u_2 \cos pt + \dots$$

Let us calculate in this way the motion in a circular cylindrical basin due to a small horizontal force acting uniformly throughout the mass of liquid, but variable with the time according to the harmonic law. The equilibrium value of h (the elevation) is evidently

$$h = r \cos \theta \cos pt;$$

and the only difficulty consists in expressing r by a series of Bessel's functions J_1 . It may be proved that

$$r = \frac{2J_1(\kappa r)}{(\kappa_1^2 - 1)J_1(\kappa_1)} + \frac{2J_1(\kappa_2 r)}{(\kappa_2^2 - 1)J_1(\kappa_2)} + \dots, \quad (V)$$

where κ_1, κ_2 , &c. are the roots of $J'_1(\kappa) = 0$, and the radius R is taken as unity. Thus the true value of h (after the motion has been going on long enough to be independent of initial circumstances) is

$$h = \frac{2n_1^2 \cos pt \cos \theta J_1(\kappa_1 r)}{(n_1^2 - p^2)(\kappa_1^2 - 1)J_1(\kappa_1)} + \dots, \quad (W)$$

the summation being extended to all the admissible values of κ . The value of n^2 is given by

$$n^2 = g\kappa \frac{e^{\kappa l} - e^{-\kappa l}}{e^{\kappa l} + e^{-\kappa l}}.$$

If the system be at rest at $t=0$, and displaced according to the law $h=r \cos \theta$ (that is, with an inclined plane surface), the subsequent motion is given in rapidly converging series by

$$h = \frac{2J_1(\kappa_1 r) \cos \theta}{(\kappa_1^2 - 1)J_1(\kappa_1)} \cos n_1 t + \frac{2J_1(\kappa_2 r) \cos \theta}{(\kappa_2^2 - 1)J_1(\kappa_2)} \cos n_2 t + \dots \quad (\text{X})$$

P.S.—Some recent observations on the periods of the oscillations of water in a large circular tank may be worth recording. The radius of the tank is 60·3 inches, and the depth about 43 inches. The oscillations were excited by dipping one or more buckets synchronously with the beats of a metronome set approximately beforehand. Soon after the withdrawal of the buckets the vibrations were counted (in most cases for five minutes), and the results reduced for a space of one minute.

Gravest symmetrical mode.—Frequency by observation 47·3. The theoretical result for an infinite depth is 47·32, and for actual depth 47·13.

Next highest symmetrical mode.—By observation, frequency = 64·1, by theory 64·02. In this case the correction for finite depth is insensible, and the length of the equivalent pendulum = $R \div 7\cdot015$.

Gravest mode with one nodal diameter.—By observation, frequency = 30·0. By theory, for infinite depth 32·81, for actual depth 30·48.

One nodal diameter and one nodal circle.—By observation, frequency = 56·0, by theory 55·8. The length of equivalent pendulum = $R \div 5\cdot332$.

Two nodal diameters.—By observation, frequency = 41·5. By theory, for infinite depth 42·09, for actual depth 41·59.

The agreement between theory and observation is as close as could be expected.

I have lately seen a memoir by M. Boussinesq (1871, *Comptes Rendus*, vol. lxxii.), in which is contained a theory of the solitary wave very similar to that of this paper. So far as our results are common, the credit of priority belongs of course to M. Boussinesq.

XXXIII. *On the Serpentinite of the Lizard—its Original Rock-condition, Methyloitic Phenomena, and Structural Simulations of Organisms.* By Professors WILLIAM KING, D.Sc. &c., and T. H. ROWNEY, Ph.D. &c.*

[Plate II.]

Its Original Rock-condition.

DE LA BECHE, referring to the “*apparent passage*” of hornblende slate into serpentine rock at Poltreath and other places, remarks that the phenomenon is “somewhat embarrassing, inasmuch as there is reason to conclude, from the grauwacke conglomerate at the Nare Point in the north part of the Lizard district, that hornblende slates were in existence, forming a surface from which large portions were abraded by the action of water, while the serpentine and diallage rocks of the Lizard were not formed; for rounded pieces of the hornblende slate, though rare, are found in the conglomerate, while no trace has yet been discovered of either serpentine or diallage rock, though the latter especially is of great hardness, and therefore quite as likely as the hornblende rock to resist comminution into fine sand, and the two former are nearer to the conglomerate than the latter.” He also observes, “in contradiction to this apparent passage, we find a mass of serpentine amid the hornblende slate between Dranna Point and Port-halla, on the north of the principal mass of serpentine, which has every appearance of having been thrust up among the hornblende slate, twisting and contorting the laminae adjoining it in directions which we should consider consistent with the passage of the serpentine in a state of igneous fusion through them. As a whole, however, the hornblende slate and rock seem to have formed a basin into which the serpentine and diallage rock have been poured in a state of fusion” †.

From observations which one of us made during a few days’ examination of the Lizard in the summer of 1873, kindly aided by Mr. W. V. Symons, manager of the Poltesco Marble Works, we are ready to admit that the serpentine rock of that district occurs both as interstratified and intersecting masses.

On the east side of Poltesco Cove there is a hard heavy rock in well-marked thick beds, composed of darkish green and greyish alternating laminae lying strictly parallel with the planes of bedding, and porphyritic through the presence of innumerable disformed crystals of a kind hereafter to be described. The green laminae consist of impure serpentine, apparently highly aluminous; and the grey ones are seemingly an

* Communicated by the Authors.

† Geological Report on Cornwall, Devon, and West Somersetshire, p. 30 (1839).

intimate mixture of felspathic and quartz matter. The laminae are regular, even, and widely continuous, so that they have quite the appearance of having resulted from sedimentation. On the other hand, there are places in the neighbourhood, especially on the shore between Poltesco and Kenwick Cove, where a rock, essentially serpentine, encloses separated masses, in beds, of semimetamorphosed sandstones and argillites, the latter possessing the character of imperfect hornblende schists.

These no doubt would be "embarrassing" cases to De la Beche; but now that important discoveries connected with serpentine have of late years been made, they cease to be such any longer. Hence we shall adopt the opinion (fully confirmed by our own researches in ophite) entertained by some of the leading mineralogists and mineralogical chemists (Bischof, Dana, G. Rose, Blum, Haidinger, and others), and disputed by only one authority of eminence*, that serpentine is in all cases the product of chemical changes or methylosis, effected in a preexisting mineral or rock of another kind, and analogous to pseudomorphism in crystalline solids. Obviously changes of the kind may take place in igneous as well as metamorphic rocks, meaning by the latter term aqueous deposits that have undergone a superinduced crystalline or structural transformation.

As regards the intersecting masses above noticed, it seems almost certain that they were originally igneous injections; and with respect to the bedded masses, such as the one occurring at the mouth of Poltesco Cove, it would seem probable that they have been in their origin argillaceous and arenaceous sediments, which became converted into hornblende schists, gneiss, and other metamorphics.

There are various kinds of igneous rocks in the Lizard: some are hornblendic or dioritic; others, in the form of intersecting dykes and intercalary beds, are pyroxenic or doleritic. We have little to say respecting the former, as time would not admit of their being properly examined on the occasion already notified. We shall therefore offer no decided opinion as to whether any of the Lizard serpentinite is a methylosed form of an igneous hornblendic rock; but it may be remarked that the hypersthene variety of the latter, occurring at Crouza and other places, seems, if itself is not a changed product†, to have been converted into the so-called diallage, common in some of the serpentinites. We have, however, to notice a fact which goes far to prove that much of the latter rock around Poltesco Cove was originally a dolerite.

* Dr. Sterry Hunt maintains that serpentine is an original chemical precipitate.

† The Crouza stone has much the appearance of having undergone a change; so that it may have been originally like the hypersthene of Volpersdorf, Moravia.

Dr. Haughton has appropriately called the rocks we are engaged with porphyritic serpentine, on account of the abundance of rude crystalline forms imbedded in its amorphous base. When large and coarsely laminated, these bodies have much the appearance of altered diallage; but in very many cases we have no doubt of their representing another mineral.

Fig. 1, Plate II., gives the outline of one of the rude crystalline forms, which are usually so crushed out of shape as to be rendered undefinable. In this example, however, we have succeeded in determining the usually modified crystal of pyroxene—a short oblique rhombic prism: the observed three faces correspond with those belonging to its front (or back)*. The crystal does not now consist of pyroxene; on the contrary, its substance has quite the appearance of chlorite, in colour, lustre, and cleavage. Fig. 2 represents a transverse section of another crystal, showing its form and chlorite-like cleavage. In many cases the latter character is rather coarse, which, with a metallic lustre, reminds one of bronzite. At first we imagined the crystals to consist of a lamellar allomorph of serpentine; but, whatever their substance may be, there can be no doubt that they are pseudomorphs after pyroxene.

The Lizard serpentine rock has its counterpart in the north of Italy—the latter being porphyritic, and charged with crushed crystals apparently similar to those just described. In the east and south-east of Europe there are igneous rocks strongly suggestive as to the original character of our home rock, viz. the wackite of Schima, Bohemia, which contains both py-

* The three faces might be mistaken for *laterals*. What gives rise to this false appearance is, that the two faces forming the bottom, also the top, (if we may be excused using such non-technical terms, applicable, however, to an *oblique* solid) of an ordinary crystal of pyroxene are generally unequally truncated, one (say, belonging to the top) being deeper or lower and consequently larger than the other. In fig. 1 the largest and deepest face forms the left slope of the top, and the corresponding one the right slope of the bottom, which peculiarity, it will be observed, causes the solid to appear inclined in this aspect. The front and back of a crystal of pyroxene are often similarly modified (that is, unequally), so that the *vertical edges* bounding the central face of the front, instead of being opposite to their analogues of the back, are usually opposite to the *faces* of this part. It is this peculiarity which has produced the vertical ridges and furrows commonly seen on the crystalline forms in the Lizard serpentine; for it will be understood that edges when opposite to each other must offer more resistance to pressure than when they are not opposite: hence it is that a face belonging to the front of these forms is *crushed in* against an edge belonging to the corresponding or opposite part, the one making a furrow and the other a ridge. These forms cannot have been originally hornblende, or the observed central face would have been less acutely rhomboidal, and it would have been *lateral*: in the usual crystal of hornblende the *front* and *back* consist each of two faces; as may be gathered from the above, each of these parts in pyroxene consists of three faces.

roxene and hornblende. But we entertain a suspicion that the porphyritic pyroxenic rock (a hard wackite) of Bufaure in the Val di Fassa, Tyrol, which contains crystals of pyroxene of the average size of the Cornish pseudomorphs, is a still nearer counterpart. It must also be borne in mind that at Monzoni, in the same region, porphyritic pyroxenite passes into serpentine rocks, some of which contain crystals in various stages of pseudomorphism, ending in perfect serpentine.

The Lizard serpentine is not characterized with many structural varieties or allomorphs. Chrysotile, which is the only one we are acquainted with, does not occur to any extent. Thin veins of it, variously intersecting the serpentines, are to be seen passing from amorphousness, through the incipient, into the fully developed or normal condition. Fig. 3 represents a vein subdivided into three layers in different states: *a*, layer of incipient chrysotile, whose substance, in being simply marked at distances (as in stylolitic structure) with faint dark parallel lines, is still manifestly serpentine; *b*, layer of normal chrysotile, which is a mass of white glistening parallel fibres, so *infinitely divisible* that it is impossible to determine their form, any more than that of the fibres of asbestos, with the highest magnifying-power*.

As yet we have failed to detect any varietal examples of chrysotile with a definite acicular structure, such as occurs in many ophites, and more or less separated by films of calcite. In one instance calcite is present in the layer *c*, but dividing it in two halves.

Its Methyloitic Phenomena.

Not only may it be assumed that the serpentinite of the Lizard was originally a different rock which became methylosed, but we have every reason to believe that its essential mineral constituent, in itself, has undergone further changes of the kind.

Many of our specimens, notably from Kynance Cove, in addition to their various shades of green (from pale to nearly black), show the serpentine gradually changing into brown, pale yellow, and cream-colour. Often the change is manifested by the serpentine becoming sinuously banded like chalcedony in agates, the bands of different colours and various degrees of lustre blending, or abruptly meeting, with the most beautiful effect (fig. 4). This is heightened by the substance of the bands occasionally appearing as if it had been in a gelatinous

* Although the incipient and normal kinds of chrysotile may be distinguished from each other, as in the text, yet the one is occasionally convertible into the other by mechanical appliances, as rubbing; which reminds us of double-refracting calcite, whose cleavage, to all appearance *nil*, is developed by a blow of the hammer.

condition. Very frequently green serpentine occurs in isolated pieces of various forms (often angular), and of all sizes down to that of a pin's head, separated by a paler-coloured variety: the latter has the appearance in many cases as if it had run into the separations.

In most cases the pale-coloured mineral is saponite, which differs from serpentine simply and essentially in containing a multiple more of water; nevertheless saponite is generally taken by mineralogists to be a chemically changed product. We may therefore safely look upon the Cornish examples under notice, considering their associations and mode of occurrence, as having originated from serpentine through the latter becoming charged with additional water*.

Assuming the conversion of serpentine into saponite, are we to consider that the change is never carried further than the production of a mineral silicate? that this latter cannot be replaced by some other mineral with a different acid? This would be in opposition to the teaching of the numerous instances known as pseudomorphs, and contrary to the opinion we have expressed on former occasions. The evidences hitherto adduced by ourselves are with difficulty appreciated, on account of their requiring the most careful microscopic observations. We are now, however, enabled to bring forward evidences which require no more than a hand magnifier to be observed with their full meaning.

One most distinctive peculiarity which separates the Lizard serpentinite from a number of other rocks of the kind, occurring in other places, is the absence in it of the variety called ophite: consequently the intimate association of calcite and dolomite with serpentine, as in the latter rock, is exceedingly rare in the Lizard. The few exceptions we are acquainted with were only obtained after considerable delay and difficulty.

In a few of our specimens showing the serpentine passing into saponite, the latter mineral is occasionally affected with a double divisional structure—an imperfectly developed form of cleavage, its two directions intersecting each other obliquely and at an acute angle†. In certain places a mass of saponite is defined on one side by a zigzag edge; next to which there is a mass of calcite containing a number of ridges, in the shape of the figure 7, formed of saponite, separated from the zigzag edge, and having their two limbs strictly parallel to the two directions of the latter (see fig. 5). Besides, the two direc-

* Serpentine consists of Silica 43, Magnesia 44, Water 13. According to Haughton, Cornish saponite contains Silica 42·28, Magnesia 29·70, Alumina 7·21, Water 18·92. Serpentine, it must be noted, often contains Alumina.

† Serpentine is occasionally affected with a similar cleavage. See Proc. Roy. Irish Acad. vol. x. pl. 43. fig. 8.

tions of the zigzag edge and the two limbs of the ridges strictly correspond with the two cleavages above mentioned. Obviously, then, there is an intimate connexion between these three formations; and no one can observe the ridges without feeling convinced that they were once integral portions of the adjacent mass of saponite, also that the presence of calcite in the intermediate spaces is an interpolated phenomenon. Moreover, on further consideration another conclusion irresistibly forces itself into notice—that the intermediate spaces were once occupied with saponite, which, having been removed, has undergone replacement by the calcite now occupying them. As in the case of an outlying reef of rock that has been separated from the mainland by the gradual removal of an intermediate mass of rock, and the latter replaced by the sand now occupying the denuded space, so it must have been with the outlying ridges of saponite and the calcitic interspaces.

It may be suggested with regard to the latter interspaces, that they are the result of cracks, widened by some cause or other, and subsequently filled up with a deposition of calcite; but this is completely disproved by their being shaped like the ridges. The portion corresponding with, say, the downward limb of the ridges might be produced by a widened crack; but in such a case the origin of the other or transverse portion must be the same. Again, the slightest reflection on the integrity, relative position, and uninterrupted connexion of the two limbs of the ridges must deprive the suggestion of every probability.

Guided by what we have very frequently observed in ophite (clearly leading to the conclusion that serpentine has been gradually replaced by calcite), we have no hesitation in offering, in general terms, the same explanation for the case under notice*.

* We would suggest the following as the particular way in which it may have been produced. As the saponite is *intermittently cleaved*, it is consequently liable to be disintegrated or decomposed here and there where the cleavage is present, and to retain its compact condition where there is no such divisional structure developed. Let it be conceived that a mass of saponite was intermittently cleaved, as in the annexed woodcut (which it will be seen is a *fac simile* in outline of fig. 5); the *cleaved* portions will thus be rendered easily affected by dissolving agencies (*e. g.* a carbonated solution), so that while their saponite is removed and replaced by calcite, the same substance will remain untouched in the *uncleaved* portions.

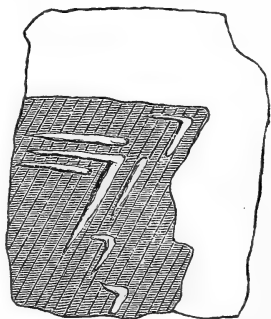


Diagram showing a mass of saponite with cleavage intermittently developed. The non-lineated portions consist of uncleaved saponite.

Similarly demonstrative is another fact to be mentioned. The mineral coloured red in our Plate, which we provisionally consider to be a variety of tremolite*, consists, in the specimen represented in fig. 6, of a mass of divergent fibres that originated in the outer layer, *a*; the mass is closed in by serpentine, saponite, and calcite, the latter being in immediate contact with its right side. It will be observed that there is an absence of the outer layer on *this side* of the tremolite, and yet the characteristic fibres are present in the adjacent portion of the mass. What have these fibres originated from? Clearly from the same layer as all the others. Then can we do otherwise than amplify our answer by stating that the layer in which they had their origin has disappeared? And are we not equally warranted in unreservedly maintaining that the layer has been simultaneously replaced by the calcite occupying its place?

Serpentine rocks occurring in other places have furnished us with numerous evidences of the same kind resulting from chemical changes, but in other minerals, as serpentine and malacolite. Examples of chrysotile (an allomorph of serpentine) changing into calcite have also occurred to us in ophites from Connemara and Italy†: in the latter instances the change had not destroyed the *fibrous structure* of the replaced mineral. Veins of fibrous dolomite are not uncommon in the Cornish serpentine; but we have not succeeded in procuring any which we can affirm to be pseudomorphic after chrysotile, though a strong suspicion may be entertained that the veins referred to are of this nature. As somewhat countenancing such a suspicion, we have the fact that these veins occur intersecting saponite, and that, while some of them are indefinitely fibrous like chrysotile here, they are decidedly columnar or prismatic there, and without any structure of either kind in another part: in the latter case the mineral is more or less affected with rhombohedral cleavage. Fig. 7 represents a thin vein in these three conditions, as displayed in specimens collected near Kenwick Cove.

But though admitting that the last cases are not directly to the purpose in hand, we must take a different view of some yet to be briefly noticed. Fig. 8 represents two cylindrical rods composed of saponite; fig. 9, other four of serpentine: both

* This mineral is usually of a reddish colour, white where it is granular. Amorphous green serpentine is occasionally intermixed with it. A qualitative analysis (the only one we have succeeded in obtaining) afforded silica, alumina, magnesia, and lime, which, with a divergent fibrous structure and the absence of water, leads us to suggest the identification given in the text.

† Proceedings of the Royal Irish Academy, vol. x. pl. 42. fig. 6; pl. 44. fig. 9.

cases are imbedded in tremolite. We have represented in fig. 10 two more rods of serpentine, but imbedded in calcite; and in fig. 11, other four composed of calcite (dissolved out by decalcification) and imbedded in tremolite. Evidently in all these cases we have different results from chemical changes; while one of them, just like those represented in figs. 5 and 6, unmistakably makes it known that calcite is one of the products of these changes.

Dr. Sterry Hunt has pronounced that we are transmutationists holding "extravagant views." Nevertheless this is no proof that we are wrong. Without going into the various evidences which have been adduced in our former papers, we would simply submit that the cases adduced in the present communication are a sufficient answer to the vivacious utterances of our esteemed fellow labourer, and that they alone are of more weight in a scientific discussion than all the ingenious elaborations, devoid as they are of any reliable and tangible evidences, which he has brought forward in support of his dogma that serpentine and other mineral silicates (steatite, hornblende, pyroxene), also the calcite associated with them, characterizing the Laurentian ophites of Canada, were "directly deposited as chemical precipitates,"—"formed by a crystallization and molecular rearrangement of silicates generated by chemical processes in waters at the earth's surface," and "not by subsequent metamorphism in deeply buried sediments" *.

* Chemical and Geological Essays, p. 300; Geology of Canada, 1866, p. 577; Quart. Journ. Geol. Society, vol. xxi. p. 70, &c. If Gümbel advocates this dogma, he lays himself open to adverse criticism in his letter, quoted by Sterry Hunt (C. G. E. p. 305, footnote):—"I do not maintain a metamorphic origin for the primitive rocks; for, although these are certainly much altered, there are no firm and consolidated rocks which are not so." [Is it to be understood, "altered like those forming the Laurentians"?] "They were formed like, for example, the limestones of more recent periods" [what limestones?]: these were once pastes, magmas, or muds; and so were the primitive rocks at the time of their origin; but during these first ages of the earth the consolidating and crystallizing forces (differing in degree only from those of the present time, and aided by a higher temperature) allowed the magma to assume the form of mineral species, more or less distinct. If we choose to call this change metamorphism, then the rocks thus formed are metamorphic; but so are the limestones of later periods." This is a strangely inconsistent way of reasoning. The "much altered" condition of a large group of rocks (diorites, gneisses, serpentinites, dolomites, &c.) is illustrated by the formation of "limestones of more recent periods." Why is there no reference to any contemporaneous argillaceous and arenaceous rocks containing substances convertible into mineral species with forms "more or less distinct"? Again, excluding such as Carrara marble, Permian and other dolomites (whose metamorphism or methylosis we have always contended for), what other recent limestones have assumed the altered or crystalline condition corresponding to that of "the primitive rocks"? Surely encrinal and other fossiliferous "limestones of more recent

Its Structural Simulations of Organisms.

In some other specimens collected at Kynance Cove the saponite encloses calcite on a small scale. It is only by immersing the specimens in a weak solution of hydrochloric acid and examining them with a magnifier that they can be seen to contain here and there patches and strings of this mineral. The vacant spaces resulting from decalcification are often crowded with projecting configurations of saponite, presenting every variety of form, including regular-shaped aciculæ, and no end in shape of arborescences.

The aciculæ, pretty uniform in length and thickness, occasionally form a regular layer investing the wall of saponite bounding a piece of calcite; and they may be in immediate contact, or separated by thin interspaces filled with calcite. In the latter condition they line one side of a fissure in the case represented in fig. 12. In their parallelism, as well as every feature, these aciculæ of saponite, it will be seen, differ in nothing except their substance from those which characterize the acicular modification of chrysotile occurring in certain ophites, and forming the so-called "nummuline layer of *Eozoon canadense*"*.

The arborescences may be simple, or highly complex—large, or small. Fig. 13 represents a medium example—which, it will be observed, closely agrees with typical examples of another eozoonal feature, called "canal system," and common

periods" are not regarded as such! It is not to be disputed that the crystalline condition of the primitive rocks has been produced by a crystallization of their *mineral* components; but any geologist must be aware that fossiliferous limestones are essentially crystalline through the organic remains they contain having become converted into calcite, and the interstitial infiltration of calcareous matter. What any sound geologist will maintain respecting the present condition of undisputed metamorphic rocks is, that it is a superinduced phenomenon, matterless what the temperature may have been that developed it; and he will reject as such the limestones just referred to. Carbonate of lime, it is well known, crystallizes with the greatest facility. Oyster-shells, with the animal still living, are often found with their valves consisting of calcite, or Arragonite. So it happens that, besides the *fossils* in the "limestones" particularized being converted into calcite, the interstices in these rocks contain the same mineral, which, in the form of a calcareous solution, had infiltrated itself into them. But obviously neither the crystallized fossils, nor the calcitic infiltrations, have any analogy with the "pastes, magmas, or muds" from which the Laurentian metamorphics have originated. Sterry Hunt and Gümbel take very little note of what may be assumed as a fact, that the Laurentians have been deeply buried in the earth's crust. Could they in this condition, aided by solutions and a high temperature, remain without undergoing transformation and transmutation?

* See 'Annals and Magazine of Natural History,' S. 4. vol. xiv. pl. 19. fig. 4; also figures of the same kind in our previous memoirs.

not only in ophite, but in chondroitic, malacolithic, and other rocks in different regions of the globe (United States, Norway, and Ceylon).

Occasionally the saponite assumes an oolitic structure, presenting spherical grains which closely resemble the ova of a fish. Fig. 14 represents a cluster of them, nestling as it were in a recess, as if they had been purposely or instinctively placed there, and had afterwards got covered up with a calcareous deposit. Many of the grains are in contact; others are isolatedly imbedded in the transparent calcite remaining undissolved after partial decalcification.

The saponite *in the mass* also presents appearances as if it contained organisms, entire and in fragments, distinguished by their comparative opacity in some cases and translucency in others, as in fig. 15. These are vermicular bodies—straight, curving, or tortuous—resembling wormcasts and shell-bearing annelids: one of the latter, represented by the circular body in the corner of the Plate, appears like a transverse section of a shell tube, having its interior filled with infiltrated matter. Examined by themselves these bodies are extremely puzzling as to their origin, and it must be admitted that they have much the appearance of the kind of organisms they resemble; but examined in connexion with the various forms assumed by serpentine or its varieties, their purely mineral origin becomes obvious. The group of pieces of serpentine shown in fig. 16 includes a great diversity of forms, such as it is conceivable would give rise to simulations of the kind; while the sinuous bands, some translucent and others opaque (fig. 4), would yield an abundant family of tortuous annelid-like bodies.

But if the foregoing configurations bear a striking resemblance to organics structures and organisms, what may we not say of those next to be described?

Imbedded in the saponite there are occasionally irregularly shaped aggregations of two kinds of bodies, which for the present may be conveniently termed ovoids and spheres. The ovoids, besides forming good-sized complex aggregations, occur often sparsely aggregated, and even sometimes isolated. They consist, partly or wholly, of slender attenuated diverging rods with a more or less definite form, being in many instances marked out by white opaque bounding planes which enclose a dark amorphous translucent interior. A transverse section of the ovoids, by cutting the rods across, presents a singularly reticulated appearance under a magnifying power of 37 diameters. When the rods are large the reticulations are decidedly angular; small rods give rise to a circular reticulation (fig. 17). The ovoids consequently appear as if their compo-

nent rods were white-walled tubules that have become filled with a translucent material. This is particularly the case in those ovoids composed of slender rods: indeed it often requires more than the eye to determine that the rods are solid; for in many instances they appear like actual tubules in an empty condition!

In short, looking at the ovoids, whether a transverse or longitudinal section (fig. 18) be under examination, they might be readily taken for fossil corals of the genus *Stenopora*; and some resemble certain Carboniferous and Permian species so closely that a consideration of structural characters alone does not enable one to differentiate them from the organisms they simulate.

The spheres have also a strikingly organic appearance. Some specimens resemble, when a few are clustered together, certain foraminifers—notably *Rotalia* and *Globigerina* (fig. 19*); while others in an isolated state have the resemblance of Orbulinas. Nay, have we not an appearance of actualities of the latter represented in fig. 20? As shown in this figure, two examples appear to be perfect casts in serpentine†; and all the others have what appears to be their shell-case not only beautifully preserved, but riddled with pseudopodial pores!

Besides the ovoids and spheres, the saponite contains patches consisting of what may be taken for closed-cell structure; and in a few cases, as in fig. 21, we have detected another structural form, resembling, in being canaliculated, sponge-tissue, with its walls appearing as if distinctly perforated.

It may be that the explanation we have to offer to account for the origin of the coral-like bodies is not strictly correct; and it is possible that we are as far from the mark regarding the foraminifer-like spheres: we also confess our inability to offer any solution of the sponge-like structure free from grave objections. Unlike the simulations in saponite, those last described do not appear to have resulted from chemical changes, but rather, to some extent, from structural modifications of the mineral we have provisionally assumed to be tremolite. Observe in fig. 22 a specimen of this mineral in its fully developed condition, consisting of prismatic and fibrous fasciculi, roughly speaking, ovoidal in shape. In some fasciculi their internal structure is well marked; while in others it is scarcely to be

* The specimen figured appears as if it were a section of a *Globigerina* with its chambers containing two or three embryos (black spots)!

† The centre of these two spheres is in the condition of saponite. The other spheres are greenish white in colour; evidently due to the serpentine passing into the flocculent condition. All the spheres are imbedded in the tremolite.

observed, so that their interior is amorphous and translucent. Again, where the prismatic structure is well defined, the prisms, especially if they are large, have their interior in the same condition*. These differences are clearly assignable to different degrees of fibrous and prismatic development.

Why some fasciculi are unlimitedly divided by which they are made eminently fibrous—why some are formed of well-defined prisms, and others, instead of being fibrous or prismatic, are entirely amorphous—we shall make no attempt to explain; but it may be remarked, with some confidence, that the ovoids resembling *Stenopora* are each a fasciculus of prisms with their interior in the amorphous condition. The remarkable appearance of empty tubules in the ovoids that are finely prismatic we assume to be caused by the interior of the prisms being imperfectly illuminated: opaque walls would necessarily intercept the light in its passage into the interior of such small prisms.

With respect to the spheres, we would suggest that they are each the nucleus, in amorphous serpentine, of a mass of radiating prismatic tremolite; that the prisms radiated from and originated on the surface of the nuclei, and were characterized by opaque walls and a translucent interior; that on the surface of a nucleus, at their point of attachment, the internal translucency of the prisms produced the perforated appearances, and their opaque walls the partitions which define the apparent perforations.

We cannot conclude without remarking that, organic-like as the foregoing cases in tremolite appear to be in our specimens, they are even more so than the figures we have given convey an idea of; indeed, although our best has been done to give a faithful representation of them, our efforts have fallen far short of depicting with sufficient exactness their wonderful resemblance to the organisms which they simulate.

EXPLANATION OF PLATE II.

*** It will be understood that the figures represent things exhibited in a number of specimens. Although this is the case, they are always represented in their own matrix; whether it be serpentine, saponite, calcite, or tremolite. The calcite having been slightly dissolved out, has its surface below that of the other minerals. The four colours—green, fawn, pale blue, and reddish—respectively represent serpentine, saponite, calcite, and tremolite. Where any of these minerals are in the *granular* or *floculent* condition, as often is the case, they are *white*; and consequently such cases are represented in this colour.

Fig. 1. A pseudomorph, imbedded in serpentine, after a crystal of py-

* Hexagonal prisms of calcite occasionally have a translucent interior and an opaque exterior, but not due, as in tremolite, to structural variations.

- roxene, partly restored. It has the colour, lustre, and cleavage of ordinary chlorite. Slightly magnified.
- Fig. 2. Transverse section of a similar crystal, showing its lamellar or cleavage structure. Slightly magnified.
- Fig. 3. Vein of chrysotile composed of three layers: *a*, chrysotile in the incipient stage, *i. e.* green serpentine slightly fibrous; *b*, in the fully developed or asbestiform condition; *c*, a layer altered to such an extent that its structure is nearly destroyed; it is divided in the middle by a film of calcite. Magnified 37 diameters.
- Fig. 4. Bands of serpentine differently coloured. Slightly magnified.
- Fig. 5. Configurations shaped like the figure 7, consisting of saponite in a granular condition, and consequently of a white colour. They are ridges imbedded in calcite, but rising above the surface of the matrix in consequence of its being partially dissolved.
- Fig. 6. A mass of mineral provisionally identified with tremolite, having one side (right) removed, as shown by the band *a* (everywhere else seen) being absent. This case may be regarded as proving that the calcite occupying the place of the absent band has replaced the tremolite which composed it. The white diverging lines are much finer, and more numerous than are represented in the figure. Magnified 37 diameters.
- Fig. 7. A vein of dolomite in three structural conditions—indefinitely fibrous, prismatic, and with rhombohedral cleavage. Slightly magnified.
- Fig. 8. Piece of tremolite enclosing two cylindrical rods (seen on their transverse section); their interior is composed of saponite, and their exterior of serpentine. Through an oversight, the exterior has not been coloured green in the figure. Magnified 37 diameters.
- Fig. 9. Piece of tremolite enclosing four cylindrical rods composed of serpentine. Magnified 37 diameters.
- Fig. 10. One cylindrical rod composed of both white-granular and green serpentine, and another apparently with all its serpentine in the white-granular condition. Both are fixed in calcite. The serpentine in the centre of the upper figure is in a granular or flocculent state, and ought to have been coloured white. Magnified 37 diameters.
- Fig. 11. Piece of tremolite enclosing four cylindrical rods composed of calcite: they are now represented by empty spaces, in consequence of their component mineral being dissolved out. Magnified 37 diameters.
- Fig. 12. Fissure-like opening in saponite containing calcite: the saponite on one side of the opening is in the acicular condition. The aciculæ, some separated and others in contact, projected into the calcite before it was dissolved out. Magnified 37 diameters.
- Fig. 13. Cavity in saponite containing calcite, in which is imbedded a dendroidal configuration composed of white granular saponite. The configuration, which is attached by its right extremity, rises above the surface of the calcite through the partial removal of the latter mineral by decalcification. Magnified 37 diameters.
- Fig. 14. Cluster of spherical bodies (resembling ova) composed of white granular saponite, and imbedded in calcite: some are above the surface of this mineral; others, faintly seen and isolated, are imbedded in it. Magnified 37 diameters.
- Fig. 15. Vermiform and other bodies imbedded in saponite; some are translucent, others white opaque, and a few are in both conditions. Slightly magnified.
- Fig. 16. Vermiform and other bodies composed of serpentine and imbedded in saponite. Slightly magnified.
- Fig. 17. Transverse section of a mass of *Stenopora*-like tubulation formed

of tremolite, and imbedded in saponite: the smallest tube-like forms appear as if they were empty. Magnified 37 diameters.

Fig. 18. Longitudinal section of the same (at the part lying immediately in contact with calcite). Magnified 37 diameters.

Fig. 19. A body composed of tremolite simulating a *Globigerina*. Magnified 37 diameters.

Fig. 20. Spherical bodies composed of flocculent (white) and amorphous (pale green) serpentine, with a perforated exterior resembling the shell of *Orbulina*. Two have a core of saponite (fawn-colour). These bodies are contained in a mass of tremolite imbedded in saponite. Magnified 37 diameters.

Fig. 21. A mass of tremolite imbedded in serpentine, and consisting of a sponge-like tissue with its walls perforated. The large dark portions represent what appear to be the mouths of canals. Magnified 37 diameters.

Fig. 22. A crystallized specimen of tremolite composed of fasciculi: its free end was imbedded in calcite, now removed by decalcification. The upper fasciculi are either fibrous or prismatic. Many of the prisms have white opaque bounding planes and a translucent interior. The lower fasciculi are altogether translucent, with the exception of their boundary surfaces, which latter form a network with large meshes. Slightly magnified.

XXXIV. On the Magnetism of Steel Bars. By Dr. CARL FROMME, Instructor in Physics at the University of Göttingen.

[Concluded from p. 204.]

§ 9. ON the occasion of these experiments I noticed some relations which were quite new to me; and they appeared so extremely interesting that I resolved to pursue them further. It was only after I had finished a great part of these investigations (which shall now be communicated in what follows), that I observed, on perusing the literature, that the same subject had already, some years since, attracted the attention of Frankenheim. His results were published in Poggen-dorff's *Annalen*, vol. cxxiii. p. 49 *et seq.*

Frankenheim proves that the *duration* of the action of a magnetizing force has no influence at all upon the amount of the residual magnetism, while the *number* of operations of the same force has a very great influence. If it has been put in operation only once, we have not thereby attained the residual moment which the same force generally (*i. e.* by repeated action) is capable of generating. The residual magnetism is increased much more by each successive operation, approximating to a boundary value, above which the same force cannot raise it.

From this I deduce the significant conclusion, confirmed by experiment, that when by repeated action of the force P the residual magnetism has reached its boundary value (the satu-

ration with residual magnetism corresponding to this force), a smaller force p cannot change it.

To all forces from zero to P inclusive, a steel bar brought to the boundary value of residual magnetism corresponding to the force P behaves like an iron bar without any coercive force.

This proposition may also be expressed thus :—

For each permanent moment of a steel bar there exist, from zero upward, a series of magnetizing forces, in relation to which this bar possesses the properties of an iron bar, or (taking as a basis the idea of molecular magnets capable of rotation, and assuming that in a bar entirely of soft iron the molecular magnets have only one position of equilibrium, but in a steel bar innumerable positions of equilibrium), expressed in the language of mechanics :—

Each of the innumerable positions of equilibrium of the molecular magnets of steel is stable for a certain range of deforming forces. The less the arrangement of the molecular magnets deviates from the direction of the forces, the more extended is the range of the forces that can be employed.

Although a proof of Maxwell's theory cannot hence be deduced, an argument in favour of it can ; for if even for the greatest deformations the molecular magnets preserve the stability of their equilibrium, provided only that their positions of equilibrium correspond to an extremely powerful permanent magnetism, one cannot see why the arrangement of the molecules which corresponds to the unmagnetic state should not also exhibit, for certain very feeble forces, a stability of the positions of equilibrium.

But, further, it is evident that, for a steel bar which is not entirely freed from permanent magnetism, in proportion to the quantity of this must also the range of the forces which leave no residual magnetism behind be more extensive. Probably it was the considerable permanent magnetism possessed by I. 5 in the experiments described in §§ 6 and 7 that occasioned the greater value of the argument K at which residual magnetism first entered. See the remark in § 7.

Now the following is what takes place in the repeated magnetization of a steel bar by a constant magnetizing force P :— The first impulse produces a residual moment M_1 ; this corresponds to a certain force ($p_1 < P$) as the moment of saturation. The second impulse increases the residual moment by $M_2 - M_1$, M_2 corresponding to a force p_2 as moment of saturation ; $p_2 > p_1$.

§ 10. The fact that a steel bar with great coercive force may, under some circumstances, behave like an iron bar without any coercive force appeared to me so important, that I resolved to

institute some experiments for the calculation of the magnetization-function k for the forces with which this happens.

After the ellipsoids had been annealed for the first time, I again made a series of experiments with the vertical component of the earth's magnetism for the ellipsoids of sort I.

	k .	K.
I. 1.	-----	-----
2.	23.7041	0.20943
3.	18.6999	0.34211
4.	16.2162	0.54715
5.	11.3674	1.09090

(The observation with I. 1 miscarried.) These values are, in consequence of the hardness being diminished by the annealing, all greater than those previously obtained. The ellipsoids were next brought by a constant magnetizing force to the moment of saturation thereto belonging; and then the magnetization-function for a descending force was calculated.

Table IV. gives the current-intensities i , the magnetizing forces X , the permanent magnetism (constant in each series of experiments) RM —the invariability of which was investigated after each single observation,—the temporary moments TM , and the magnetization-function k , with its argument K . The care with which the experiments were conducted is attested by the values of k with their slight differences.

TABLE IV.

ELL.	i .	X .	TM .	k .	K .
I. 1. RM=15860.					
	4.774	82.71	1663600	25.34	2.453
	2.262	39.19	789060	26.3	1.121
RM=79320.					
	10.29	178.2	3606000	31.86	4.231
	8.047	139.4	2819800	31.19	3.379
	4.765	82.56	1666100	28.53	2.183
	2.255	39.07	787840	27.74	1.052
I. 2. RM=255800.					
	9.921	171.9	7076400	27.25	7.287
	7.348	127.3	5162600	20.02	7.237
	5.721	99.12	4035000	21.44	5.281
	3.857	66.82	2708600	19.86	3.828
	1.795	31.09	1255500	18.64	1.891
I. 3. RM=133500.					
	5.763	99.84	7029000	16.42	8.9
	4.433	76.8	5408400	16.47	6.826
	3.303	57.23	4004600	15.35	5.421

TABLE IV. (continued).

Ell.	<i>i</i> .	X.	TM.	<i>k</i> .	K.
I. 3. RM=256300.					
	8.444	146.3	10286700	16.2	13.2
	6.916	119.8	8454000	16.82	10.446
	5.11	88.53	6236000	16.52	7.847
	3.4	58.9	4144000	16.28	5.291
	1.477	25.58	1794300	15.74	2.37
I. 4. RM=132400.					
	3.497	60.58	7603600	13.69	8.874
	2.979	51.61	6484400	13.78	7.516
	2.284	39.57	4954800	13.46	5.879
	1.792	31.05	3862000	12.87	4.792
RM=546150.					
	6.785	117.55	14914500	14.76	16.14
	5.753	99.68	12594700	14.34	14.03
	4.411	76.42	9641400	14.18	10.86
	3.461	59.96	7556500	14.07	8.582
	2.124	36.8	4608200	13.47	5.467
	1.106	19.15	2383100	13.09	2.915
I. 5. RM=539600.					
	3.509	60.8	8266600	11.4	15.31
	2.58	44.7	6041000	11.13	11.46
RM=957000.					
	6.326	109.6	14957600	11.57	27.29
	5.428	94.05	12800600	11.44	23.62
	4.231	73.31	9935000	11.25	18.64
	3.284	56.89	7690000	11.14	14.57
	2.055	35.6	4785300	10.90	9.265
	1.108	19.19	2563000	10.88	5.003
II. 1. RM=27480.					
	7.582	131.35	2635000	17.81	5.73
	5.962	103.3	2063300	16.21	4.931
	4.424	76.64	1528800	15.74	3.762
	2.488	43.1	858600	15.12	2.198
RM=43350.					
	10.127	175.45	3541000	20.66?	6.638
	9.049	156.78	3144000	17.65	6.898
	8.324	144.2	2885200	16.775	6.662
	7.213	124.96	2498400	16.524	5.856
	6.196	107.35	2140000	15.547	5.332
	5.096	88.29	1758000	15.226	4.472
	4.117	71.33	1418700	14.86	3.698
	2.996	51.91	1033300	15.13	2.646
II. 2. RM=139720.					
	8.34	144.48	5784300	11.48	13.65
	7.01	121.44	4838400	10.92	12.00
	5.51	95.45	3798000	10.776	9.551
	4.11	71.21	2843100	11.16	6.902
	2.732	47.33	1884400	10.84	4.709
	1.638	28.38	1125900	10.483	2.911

TABLE IV. (continued).

Ell.	i.	X.	T.M.	k.	K.
II. 2. RM=138030.					
	10.15	175.84	7043300	11.54	16.54
	9.358	162.13	6487700	11.423	15.39
	8.492	147.12	5881500	11.308	14.09
	7.474	129.48	5164000	11.034	12.68
	6.542	113.33	4510200	10.797	11.32
	5.396	93.49	3709800	10.495	9.58
	4.401	76.24	3022000	10.379	7.89
	3.47	60.11	2373000	9.988	6.44
	2.41	41.75	1648000	9.973	4.48
II. 3. RM=270600.					
	6.465	112.0	7649000	10.35	15.79
	4.831	83.69	5691400	10.045	12.11
	3.394	58.8	4248700	9.961	8.57
	2.128	36.86	2484500	9.465	5.61
RM=514000.					
	10.112	175.2	12111000	11.312	22.88
	8.906	154.3	10655000	11.224	20.29
	7.79	134.96	9297000	11.02	18.03
	6.624	114.77	7867600	10.63	15.82
	5.536	95.92	6562000	10.476	13.39
	4.389	76.04	5182500	10.2	10.86
	3.329	57.68	3916000	9.925	8.434
	2.32	40.19	2717400	9.65	6.013
II. 4. RM=578800.					
	6.51	112.8	14195000	13.01	17.44
	5.308	91.95	11541000	12.78	14.43
	4.147	71.85	8981600	12.455	11.53
	3.027	52.45	6538300	12.25	8.533
	1.986	34.415	4243000	11.47	5.914
RM=1190000.					
	10.026	173.7	22210000	14.48	24.52
	8.635	149.6	19065000	14.143	21.54
	7.457	129.2	16386000	13.69	19.13
	6.136	106.3	13413000	13.23	16.2
	4.895	84.81	10646000	12.79	13.31
	3.722	64.48	8045000	12.315	10.44
	2.739	47.46	5886000	11.875	7.922
	1.865	32.31	3975000	11.313	5.615
II. 5. RM=1056400.					
	6.254	108.3	14253000	10.4	29.07
	5.37	93.04	12196000	10.26	25.2
	4.174	72.32	9404000	9.965	20.02
	3.257	56.43	7260000	9.592	16.06
	2.044	35.41	4500500	9.201	10.38

TABLE IV. (*continued.*)

ELL.	<i>i.</i>	X.	TM.	<i>k.</i>	K.
II. 5. RM=1931000.					
	8·137	140·97	18805000	10·965	36·38
	7·106	123·1	16310000	10·679	32·36
	6·072	105·2	13815000	10·33	28·36
	4·957	85·88	11212000	10·114	23·515
	3·667	63·53	8194400	9·682	17·95
	2·254	39·05	4961200	9·183	11·46
RM=6213000.					
	7·979	138·23	18737000	11·684	34·01
	7·183	124·45	16746000	11·345	31·31
	6·377	110·47	14776000	11·076	28·30
	5·457	94·54	12542000	10·738	24·78
	4·650	80·56	10606000	10·43	21·57
	3·836	66·46	8672000	10·096	18·22
	3·021	52·35	6754000	9·921	14·78

§ 11. From Table IV. we readily obtain some analogies with the values of k ascertained by the earth-inductor. For here again, with sort I., the values corresponding to the ellipsoids with different degrees of eccentricity arrange themselves in a series descending as the eccentricity increases; on the contrary, with sort II. the values of k cannot be arranged in a series, but, while II. 2, II. 3, and II. 5 have tolerably equal values of the magnetization-function, II. 4 furnishes, as before, greater values. But now II. 1 also has a sensibly higher magnetization-function.

According to this let us form, in conjunction with that previously found, the following representation of the course of k :—After the variously elongated ellipsoids have reached the same (previously noted) minimum of their magnetization-function, the values of k again recede from one another in the softer steel as the magnetizing force X and the permanent magnetism simultaneously increase, while again the greater values of k belong to the less-elongated ellipsoids. With the harder steel, on the contrary, k appears to possess pretty nearly the same value: the higher values of II. 1 and II. 4 may be regarded as produced by differences in the structure of the steel. That k possesses a certain degree of variableness is probably beyond doubt; yet although the permanent magnetisms are not sufficiently varied, thus much can be seen, that this variability in sort II. is not very great. With a doubled permanent magnetism, II. 3 and II. 4 do not exhibit any change

in the k corresponding to an equal magnetizing force X , while II. 5 decidedly shows an increase in k when RM rose from 1931000 to 6213000. The reverse is the case with II. 1, where k diminishes.

Since the different series of observations were not carried out under absolutely similar external circumstances, especially not with equal distance of the helix from the magnetometer, no conclusion concerning the behaviour of k can be drawn from such slight variations, which are moreover not all in the same direction, and we shall be able to regard k as constant for the harder sort II.

In sort I. it can be proved more certainly that, when RM is quintupled, the k of I. 1 is considerably augmented, just so that of I. 3 with RM doubled, while for I. 4 and I. 5 nothing certain can be affirmed.

If we consider the results before us in their relation to Neumann and Kirchhoff's developments, we perceive that in the present case these are no longer valid: to the same argument K , with the different ellipsoids the most different values of k correspond.

Neumann and Kirchhoff's developments hold only for steel bars which possess no permanent magnetism and are submitted to but slight magnetizing forces; they at once become inadmissible as soon as the steel is permanently magnetic.

This is much more strikingly apparent with the softer than with the harder kinds of steel.

When, lastly, we examine the series of observations of Table IV. singly, we see the analogy with soft iron carried out also in the behaviour of the magnetization-function:—

For the forces which are incapable of altering the permanent magnetism of a steel bar, the number k becomes less as the force diminishes; the magnetization-function of steel exhibits the same behaviour as that of soft iron under the action of the always slight magnetizing forces which have here been employed.

(For intenser forces, as with soft iron, an increase of k would of necessity enter with a diminishing force.)

In what manner this diminution of k takes place, I have endeavoured to ascertain by a graphic representation of the dependence of k upon its argument K . I found, however, somewhat complicated curves with several turning-points, which appeared to become the more regular (viz. straight lines) the more I augmented the permanent magnetism.

Now, since by this, on account of the diminished constancy of the quantity of the permanent magnetism, the difficulties of observation are increased, and therefore properly a still more irregular course of the curves would have been to be

pulse by the number of the impulses, the saturation-moment A , and the increment B of the moment from the first to the last impulse.

The magnetization by induction-currents, on the other hand, is very well represented, he says, by the formula

$$y = A + B(1 - e^{-\alpha x}), \quad . \quad . \quad . \quad (2)$$

where A , B , and α are constants, A the moment originally present, B the total increment produced by the action of the force, and e the base of the natural system of logarithms. This formula corresponds to a much quicker increase of the residual magnetism than the first.

While M. Bouty is inclined to deduce the difference of the formulæ from the difference of the methods of magnetization, I should rather, judging from the examples he adduces, seek them in the different initial magnetic states of the steel needles. Two of his examples may serve as evidence:—

For (1).		For (2).	
Number of passages to the spiral.	Magnetic moment.	Number of direct induced currents.	Magnetic moment.
1	51·46	0	71·55
2	54·55	1	75·30
3	55·43	2	78·55
4	56·10	3	80·50
5	55·95	4	80·80
10	56·48	8	81·05
20	57·68	16	81·05
30	57·92		
50	57·75		

While, then, in the first instance the little bar had no initial magnetism (was “innocent of any previous magnetization”), and at the conclusion possessed the residual magnetism 57·75, in the second the initial magnetism was 71·55, which was raised by the action of the magnetizing force to 81·05 only. But then, according to the proposition previously advanced, the number of the requisite impulses must be less, and the augmentation of the residual magnetism follow more rapidly.

§ 13. Frankenheim’s investigation had arrived at the remarkable result that, with freshly annealed bars, the residual moment R_x attained by an impulse x stands in an invariable ratio to the moment of saturation R , independent of the dimensions of the bars, their hardness, and the strength of the force acting upon them. Now my experiments seem to prove that this holds good also for bars not freshly annealed, but having

an initial magnetism U , as soon as we count this to the residual magnetism. These experiments are represented in Table V.

TABLE V.

Ellipsoid.	U .	$\frac{U+R_1}{U+R}$.	$\frac{R_1}{R}$.	Ellipsoid.	U .	$\frac{U+R_1}{U+R}$.	$\frac{R_1}{R}$.
I. 1.	14100	0.955	0.942	II. 1.	31730 142200	0.893 0.936	0.865 0.828
2.	38530 126300 195400	0.941 0.945 0.940	0.916 0.859 0.839	2.	158750 313300 431400	0.931 0.938 0.934	0.928 0.933 0.795
3.	51540 150850 293600 589800	0.930 0.947 0.932 0.932	0.901 0.895 0.868 0.795	3.	276650 397600 448100 698600 990400	0.958 0.973 0.931 0.928 0.917	— — 0.822 0.783 0.732
4.	287200	0.968	0.930	4.	706000 897400 1039000 1519000 2317000 2865400	0.955 0.962 0.958 0.922 0.942 0.912	0.833 0.826 0.811 0.785 0.733 0.648
5.	1049200 1754700 2802000	0.929 0.918 0.906	0.827 0.789 0.670	5.	962300 1287000 1807800 2497400 4205000	0.951 0.965 0.928 0.896 0.902	0.836 0.824 0.762 0.757 0.592
Mean		$\frac{U+R_1}{U+R}$	$\frac{U+R_1}{U+R}$				
I. 1.	0.955 ?	II. 1.	0.910				
2.	0.942	2.	0.934				
3.	0.935	3.	0.941				
4.	0.968 ?	4.	0.942				
5.	0.918	5.	0.928				
		0.944	0.931				

Although this Table has no pretension to completeness, since with several of the ellipsoids there are too few experiments, and the dimensions and hardness of the bars are too little varied, yet this much is evident, that $\frac{U+R_1}{U+R}$ may pass for tolerably constant; while it is indubitably shown that $\frac{R_1}{R}$ diminishes when the magnetizing force (or what is here considered the same thing, U) increases. Moreover this diminution of $\frac{R_1}{R}$ with increasing U is self-evident from the found constancy of $\frac{U+R_1}{U+R}$.

Therefore to the progressive rotation of the molecular magnets into positions of magnetic equilibrium is opposed an increasing resistance.

A dependence of the diminution of $\frac{R_1}{R}$ on the magnitude of the difference $R-U$ or of the ratio $\frac{R}{U}$ does not follow. In opposition to this, M. Bouty, in a more recent notice*, maintains, on the ground of a series of experiments, of which he adduces the following as an example, that his ratio $\frac{A}{A-B}$, which corresponds to my $\frac{R}{R_1}$, decreases when the magnetizing force increases, according to which $\frac{R_1}{R}$ would increase. (Whether his needles were annealed before each series of experiments, or if they possessed initial magnetism, is not stated.)

Needle of 0.553 millim. diameter.

Magnetizing force.	$\frac{A}{A-B}$
10	1.280
14	1.246
18	1.165
22	1.150
26	1.115
30	1.085
34	1.058
38	1.036
42	1.025

Although here is unquestionably a diminution of $\frac{A}{A-B}$, yet our author errs fundamentally, or at least expresses himself quite incorrectly when he asserts of this ratio, "Il tend vers zéro." An approximation to *nil* is certainly out of the question; at the utmost there may be an approximation to unity.

While, then, according to M. Bouty's experiments, R_1 and R are continually coming nearer to equality, so that finally a single impulse will produce the moment of saturation, according to mine they are always receding further and further from each other, so that the saturation-moment corresponding to a force is always difficult to attain.

On what causes these opposite results depend we cannot at present say. M. Bouty's quantities are not expressed in absolute measure; and from his data it is not possible to reduce them thereto. Yet I do not give up the expectation that his

* *Comptes Rendus de l'Acad. des Sciences*, 1875, No. 13.

and my results will be found not merely not contradictory, but probably mutually complementary.

For reasons easily conceivable, which I need not further discuss, it cannot well be assumed that the ratio $\frac{R_1}{R}$ continues to diminish till it arrives at zero; much rather it is not improbable that, as the magnetizing force goes on increasing, the ratio begins to rise again. M. Bouty, however, worked with very thin, while I worked with tolerably thick steel bars; and hence it really follows that in his, in comparison with my observations, much greater magnetizing forces were in play. At all events the subject is well deserving of further examination; and I shall not delay commencing experiments for that purpose.

§ 14. In § 9 the proposition is advanced that a force of determinate intensity cannot generate residual magnetism beyond a determinate limit. Taken rigorously, this is not quite correct—at least, not in the sense in which it was there understood; for if, by repeated action of a constant force, a steel bar be brought to the corresponding maximum of its residual magnetism, then loses a portion by concussions, or even by merely lying at rest, and thus by the action of time, and afterwards the same force be again applied, the residual magnetism now increases beyond the previous limit.

If the same process be again repeated several times, a limit must finally be reached which will not be exceeded even by further losses and fresh magnetization. This value only of the residual moment can be designated the proper moment of saturation of the force in question.

It is self-evident, and confirmed also by my experiments, that where a loss of residual magnetism has occurred in a bar which had been previously saturated by a force P , even a force p , which before, with a maximum attained, could no more than P itself produce an augmentation, can raise the diminished residual moment, and, if the intensity be sufficient, bring it back to the maximum corresponding to the force P .

§ 15. We have hitherto considered only one part of what takes place in the repeated magnetization of a steel bar by a constant force, namely the variation of the residual magnetism. But similar questions to those respecting the residual, can now be put concerning the temporary magnetism. Here three cases are to be distinguished.

The first, simplest, is that of repeated magnetization by the same current, which, however, has never previously acted on the rod (§ 9). The question is:—Does the temporary magne-

tism, like the residual, increase with each successive impulse, remain constant, or diminish?

In order to contribute to a correct judgment on the degree of accuracy which an answer to such questions concerning the temporary magnetism can possess, I would remark that the latter, as is well known, may fluctuate, always within certain but, with soft iron, tolerably wide limits, even when the current is constant.

Nevertheless, on the ground of numerous experiments, I believe I may assert that *The temporary magnetism of a steel bar diminishes on the bar being repeatedly magnetized by a constant current, but so that the total magnetism remains unaltered; therefore what is gained in residual, is lost in temporary magnetism.*

The second case has been already treated in § 11. We there had the same ellipsoid in different magnetic states, with different RM, submitted to the same force; and we inquired how the magnetization-function behaves with an increasing permanent magnetization. To this question we could make no definite answer; k , and with it the temporary magnetism, in part remained constant, in part increased, and in part even diminished.

From this it is now evident that an answer to the third case, given by the last preceding section, cannot but be extremely difficult. In fact, on making some experiments, I obtained contradictory results, sometimes greater, sometimes smaller temporary moments, while the residual magnetism, in accordance with the proposition advanced in § 14, always proved greater.

Göttingen, September 1875.

XXXV. On Crookes's Radiometer.—Part II.* By G. JOHNSTONE STONEY, M.A., F.R.S., Secretary to the Queen's University in Ireland.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

Hampstead, March 18, 1876.

MANY persons have told me that they have found it difficult to understand the explanation that I offered in the March Number of the Philosophical Magazine, of the pressure which Mr. Crookes has discovered. I have therefore endeavoured to present a picture of the mechanism by which I conceive the pressure to be produced, in a form which will, I

* Being the substance of part of a communication made to the Physical Society, March 11, 1876.

hope, be intelligible to persons who have not made a special study of the dynamical theory of gases; and as the subject is one not easily illustrated in this way, I hope the result of my attempt will be deemed of sufficient permanent interest to deserve a place in your columns.

I am, Gentlemen,

Yours faithfully,

G. JOHNSTONE STONEY.

[The numbering of the paragraphs is continued from the paper on the same subject in the March Number of the *Philosophical Magazine*.

Readers of that article are requested to make the following corrections in it:—

In paragraph 3 (p. 178), instead of "I will assume that the pressure within the chamber is 0·1 of a millim." read "not much short of 0·1 of a millim."

In paragraph 4, instead of " w_1, w_2 ," read " $w_1 - w_2$."

In paragraph 7 (p. 180), instead of " $\frac{1}{10,000}$ of an atmosphere," read "about $\frac{1}{10,000}$ of an atmosphere."

In paragraph 9 (p. 181), instead of "We may further regard" read "We may further without risk of serious error regard."]

10. When light is allowed to fall on a Crookes's radiometer, there are two stages of the phenomenon to be noted and carefully distinguished.

11. The first is a brief period of adjustment which arises when the light first falls on the blackened face of the disk. During this stage the different parts of the attenuated air within the little chamber assume the conditions which correspond to the presence of the warm body. This adjustment is made with great rapidity, and may occupy perhaps the $\frac{1}{2500}$ of a second. This is about the time that would be required if this adjustment is made at a speed comparable with the velocity of sound; and this is likely, since the velocity of sound is the speed with which a somewhat analogous adjustment is propagated in air.

12. The second stage of the phenomenon is the permanent state which supervenes when the period of adjustment is over. And here there are two cases to be distinguished. The first is that which occurs when the exhaustion is insufficient. In this case (leaving gravity, and consequently convection currents, out of consideration for the present) a thin layer of air in contact with the disk has been both warmed and rarefied during the period of adjustment; while beyond

this layer the air throughout the greater part of the chamber will be very slightly denser and warmer than before the adjustment. There will also be another layer of varying temperature and density in contact with the glass, having on one side the temperature of the air in the little chamber, and on the other the somewhat lower temperature of the glass. This is the state which will come into existence during the adjustment, and which will then continue as the permanent state of affairs so long as the disk and glass are maintained at their respective temperatures. While this is the condition of the apparatus, there would be no transfer of heat from the disk to the glass if convection-currents were absent: the contrivance cannot act as a heat-engine; and there is accordingly no pressure of the kind which Mr. Crookes has detected, and to which I would suggest that we should give the name of Crookes's pressure.

13. On the other hand, if the exhaustion be carried further, the layers of varying temperature in contact with the disk and with the glass will become thicker; and if the exhaustion proceed sufficiently far, these layers will be so dilated that there will not be room between the disk and the glass for their full development; and then will arise the state of things in which Crookes's pressure presents itself. The following description will give an idea of what is now going on within the chamber. As soon as the brief period of adjustment is over a procession of swift molecules at once commences, which is ever starting from the front or warmed side of the disk (the heater of the little engine), and jostling its way to the opposite hemisphere of cool glass* (the cooler of the heat-engine). Another procession is as constantly starting from the cool hemisphere of glass and working its way both up to the disk and to the regions behind the disk. These processions last so long as the heater and cooler are maintained at their respective temperatures. Any member of the slow procession that comes into contact with the front of the disk is, in rebounding, converted into a member of the swift procession; and any member of the swift procession that comes into contact with the glass is converted into a member of the slow procession.

14. To complete the picture we must suppose the members of both processions to be mixed up with a crowd of molecules moving about irregularly, and not belonging to either procession. The processionists are so buffeted that probably no one follows his own procession far. But some of the molecules against which he jostles generally carry a part of his

* If the instrument has four arms, the back of one of the other disks at right angles is to be regarded as part of the cooler of the little engine.

momentum somewhat further. In this way the characteristic of each procession is retained over a considerable distance, although it is gradually effaced as the procession advances. As the swift procession advances it becomes less swift; as the slow procession advances it becomes less slow. If there is room between the disk and the glass for the *entire* gradient, that is, for the whole thickness of those layers which I have described, then that part of the slow procession which reaches the disk will have not only lost its sluggishness, but will have attained the full molecular speed of the beginning of the swift procession; and the swift procession, where it comes into contact with the envelope of glass, will have become as slow as the beginning of the slow procession. In such a case there would be no Crookes's pressure. But if there is not room for the full gradient, then each procession will retain a portion of what characterizes it when it reaches its destination. The front of the swift procession, where it reaches the glass, will still retain an average of molecular motions more rapid than that which corresponds to the temperature of the glass; and that part of the front of the slow procession which reaches the disk will have its internal motions less active than those corresponding to the temperature of the disk, while molecules of a still more sluggish type will alone reach the back of the disk and the hemisphere of glass behind the disk.

15. Here, then, we have conditions which would produce a difference of pressure on the front and back of the disk, *if* equal numbers of molecules had access to both sides. However, the number striking the disk in front will not be quite so great as the number striking its back, because the members of the swift procession will shove back some of those which otherwise would have made their way up to the disk. But they do not keep back enough to prevent there being *some* difference of pressure; *for it would require the whole gradient to produce such a defect of density as would entirely compensate for the greater average momentum imparted by each molecule that strikes the front, as compared with the momentum imparted by each molecule striking the back of the disk.* This is the cardinal point in the explanation.

16. As this is the key to the whole explanation, and as I find that some who feel an interest in the subject find a difficulty in apprehending it, I will give a proof of it from another point of view, and with every necessary detail. Let us imagine a large apartment containing throughout air as attenuated as the air in the radiometer. Let its temperature and the temperature of the walls of the room be t , and its tension p grammes per square centimetre, p being a sufficiently low frac-

tion. Let v be the average velocity of molecules of air at the temperature t . Then this air presses against the walls of the apartment with a force of p grammes per square centimetre, because its molecules strike the walls with velocities of which the average is v , and rebound from them with velocities of which the average is also v . Let n be the number of molecules which on the average strike a square centimetre of the walls per second. Then, if we picture to ourselves a plane drawn anywhere through this medium, n molecules per second fly through every square centimetre of this plane from left to right, and an equal number fly through it from right to left. Having acquired these conceptions, let us next suppose a disk like the disk of a radiometer, to be situated in the middle of this apartment, the front of the disk being kept at temperature $t + \Delta t$, and the back at temperature t . Let us further suppose that there is no gravity, in which case there will be no convection-currents. But a complete gradient would promptly form in front of the disk; in other words, the air in front of the disk would be warmed to a considerable distance, and somewhat dilated. Throughout this gradient the temperature would vary from $t + \Delta t$ on the one side, to t on the other. The density would also vary, and in such a way that the tension would be everywhere the same as in the rest of the apartment, viz. p grammes per square centimetre. In this case the disk would be pressed equally in front and behind. Let us next form the conception of a spherical surface, with its centre at the middle of the disk, large enough to contain the disk and extend beyond it, but not so large as to reach through the gradient. The hemisphere in front of the disk would thus contain air belonging to the gradient, and the hemisphere behind the disk would contain air of the same kind as in the rest of the apartment. Through each square centimetre of the surface of the hemisphere behind the disk there will be n molecules per second flying into the sphere, and n molecules per second flying out; and these molecules will be endowed with velocities of which the average is v . But through any square centimetre of the hemisphere in front of the disk there will be a number of molecules flying in and flying out per second which will be less than n , while their average velocity will be greater than v . Let us next suppose that this spherical surface, while it still allows the molecules to pass through it and retain the direction of their flight, is given the property of being able to cut down the velocities of molecules whose average is above v , so that the average velocity of the molecules after passing through the surface becomes v , whatever it may have been before. This new property will not make any dif-

ference in the state of things behind the disk ; but it will cause a new adjustment to be necessary in front. In fact the part of the gradient which lies outside the sphere will cease to exist, since there is nothing now to prevent air of the same pressure and density as in the rest of the apartment from coming quite up to the sphere. Accordingly, when the little instant of adjustment is over, this will be the state of things outside the sphere. Hence, when the adjustment is over, there will be n molecules flying *inwards* through every square centimetre of the front hemisphere. Now the density can no longer undergo change when the adjustment is over ; and accordingly there must be as many molecules passing one way as the other. Hence we find that after the adjustment, n molecules per second must come up to every square centimetre of the inside of this hemisphere and demand an exit through it, which is accorded to them on the condition of their surrendering a part of their momentum as they fly through the partition. For since the gradient is now incomplete, the motions inside will keep up an average velocity which is above v in spite of the incursion of the strangers. This surrender of momentum means that there is a pressure against the inside of the hemispherical shell, which does not exist against the outside. Now as the same number of molecules pass in as pass out through every part of this hemisphere, we are at liberty to render the shell impervious to molecules, if we substitute for its hypothetical property the following, which can be easily attained in practice, viz. the property that, although molecules may come up to the shell with velocities of which the average is above v , they shall rebound from it, and in rebounding shall be sent off with velocities of which the average has been reduced to v . This only requires a solid shell which is maintained at the temperature t . This shell suffers on the outside a pressure of p grammes per square centimetre, and within a pressure of more than p grammes per square centimetre ; while if a similar impervious shell at the same temperature is substituted for the hemisphere behind the disk, it suffers a pressure of p grammes per square centimetre both on the inside and outside. The final change may now be made. The air outside the impervious sphere may have its pressure increased and become the ordinary air of a room without disturbing any part of what goes on within the shell ; and we have now an apparatus which is in fact no other than the radiometer with its glass containing-vessel maintained at temperature t .

17. Hence there is pressure on the inside of the front hemisphere of glass which is in excess of the pressure on the inside of the hemisphere behind the disk. And there is of course

also pressure on the front of the disk in excess of the pressure on its back. These excesses of pressure are what I have called Crookes's pressure. From the mechanical principle of momentum it follows that their resultants must be in a right line (which is, of course, perpendicular to the disk), equal in amount, and opposite in direction.

18. It has been urged as an objection to this explanation that the arms of a radiometer often fly round so fast that their revolutions cannot be counted, and that they appear to the eye like a haze. It has been supposed that the vanes, in sweeping round so rapidly, would mix together all strata of air within the instrument, so as to prevent the development of the processions which I have described. But this difficulty will disappear when we appreciate the exceeding activity of molecular motions, and the exceeding shortness of that first stage of the phenomenon which I have called the period of adjustment. It probably occupies, as I have already observed, something like the $\frac{1}{2500}$ of a second of time. After the lapse of some such mere moment of time, the machinery is in full operation, so that the vanes might probably whirl round even hundreds of times in a second without stopping its action.

19. Again, it has been supposed that the rarefaction in the vacuum-chamber is carried very much further than I have admitted in my former paper. Considerable latitude in regard to this is allowable; and it will therefore be desirable to examine what are the probable limits of tension within which Crookes's pressure is likely to be appreciable.

20. It must be remembered that when the disk is vertical, as in a radiometer, Crookes's pressure will be accompanied by the forces occasioned by a convection-current. The convection-current will influence the motion of the vane in three ways. It will bring a continuous supply of cold air to the front of the disk, which, being thrown off with augmented molecular motions, will cause a recoil of the disk in the same direction as Crookes's pressure. The convection current will also occasion a defect of tension in front of the disk, by that law of fluids in motion according to which the tension decreases along a stream wherever the velocity increases. This will occasion a minimum of tension where the velocity of the convection-current is greatest, *i. e.* in front of the disk, and will thus give rise to a force opposed to Crookes's pressure. And again, the convection-current, after being turned downwards by the roof of the little chamber, is likely to become a draught blowing upon the back of the disk with a force which is also opposed to Crookes's pressure. The resultant of these

three forces seems in all the experiments to have been opposed to Crookes's pressure.

21. At any particular degree of exhaustion it is only the difference between this resultant and Crookes's pressure which becomes apparent. It is easy to see that as the rarefaction proceeds convection-currents will become in some degree more active, but this occurs in a degree sufficiently moderate to allow the resultant force to diminish continuously as the exhaustion proceeds. On the other hand, what I have called Crookes's pressure does not exist at all until such a degree of rarefaction has been attained as will cause the gradient to extend from the disk beyond the walls of the chamber; after this is once effected, if the exhaustion be still continued, Crookes's pressure first rises to a maximum and then steadily diminishes. It is evident, therefore, that there is a certain tension, depending in some degree upon the form of the radiometer, at which the excess of Crookes's pressure over the force arising from the convection-current will be a maximum.

22. If we assume that the chamber is of such a considerable size as compared with the size of the disk that the general temperature of the air within it is but slightly raised above that of the glass, the following formula will approximately give the value of Crookes's pressure:—

Crookes's pressure per square centimetre

$$= \frac{1}{2} \frac{\delta - d}{\delta} p \cdot \frac{\Delta t}{273 + t},$$

where \equiv means is nearly equal to, and where p is the outstanding tension per square centimetre within the chamber, δ^* the length of gradient corresponding to p and Δt , d the distance of the disk from the glass in front of it, t the temperature of the glass and of the back of the disk, regarded as approximately the same, and finally Δt the excess of temperature imparted to the front of the disk.

23. Let us suppose, as in my former paper, that $\frac{\delta - d}{\delta}$ will be equal to $\frac{2}{3}$ † when p is $\frac{1}{10,000}$ of an atmosphere. Then the

* The value of δ at any tension is given by the formula

$$\delta = k \left(\frac{1033}{p} \right)^{\frac{4}{3}} \Delta t,$$

where k is the length of gradient, *i. e.* the thickness of the thin layer of warmed air, when air at the temperature t and at the pressure of one atmosphere is in contact with a body whose temperature is $t + 1^\circ$ (see *Philosophical Magazine* for last month, p. 180, § 7). The assumption I venture to make in §§ 6 and 22 amounts to this—that k is as much as $\frac{1}{200}$ of a millimetre.

† This follows from the assumption referred to in the last footnote.

value of this fraction will scarcely differ from unity when p becomes $\frac{1}{100,000}$ or $\frac{1}{1,000,000}$ of an atmosphere. Using these values for $\frac{\delta-d}{\delta}$, we can compute by the foregoing formula how much the front of the disk would need to be warmer than the rest of the apparatus at these several tensions, to produce a Crookes's pressure of the hundredth of a milligramme per square centimetre, which is an amount Mr. Crookes observed.

24. We thus find that if there were no convection-current we should produce this pressure by warming the front of the disk about 0.09 of a degree at a tension of $\frac{1}{10,000}$ of an atmosphere, while an elevation of 0.6 of a degree would be required at a tension of $\frac{1}{100,000}$, and an elevation of 6° at a tension of $\frac{1}{1,000,000}$ of an atmosphere. This last excess of temperature may be looked upon as inadmissible, since it is not likely that the front of the disk is ever so much as 6° warmer than the back. Hence we may, I think, conclude that where Crookes's pressure is conspicuously* present, the exhaustion does not approach the millionth of an atmosphere †.

25. The probability seems to be that the effects of convection-currents are sufficiently feeble at tensions between $\frac{1}{10,000}$ and $\frac{1}{100,000}$ of an atmosphere for us to fix on some tension between these limits as the tension at which we are likely to find the greatest apparent effects, *i. e.* the greatest excess of Crookes's pressure over the resultant of the forces due to convection-currents.

* It must, however, be remembered that a force very much less than the hundredth of a milligramme per square centimetre would manifest itself by causing motion in a radiometer.

† This would afford a means of ascertaining whether there is any residual effect not accounted for by the foregoing explanation, if we might depend on the methods by which very low tensions are determined. But it is evident that a gas which can be occluded in any of the solids present, or absorbed in any liquid, or condensed upon the surface of either, or any trace of vapour which either the solids or liquids can give off, will at a certain point of tenuity elude detection. All direct methods are on this account so untrustworthy that I should be disposed to think that low tensions may be best estimated by observing the amount of torsion which the Crookes's pressure in them can give to a fibre of glass or silk, and comparing it with the formula in § 22, using unity for $\frac{\delta-d}{\delta}$ when the tension is excessively low. A gauge might easily be constructed upon this principle.

XXXVI. *A new Arrangement for the Micrometer of the Automatic Spectroscope.* By WALTER BAILY*.

IN some spectroscopes the micrometer-wires are seen by means of a pencil of light reflected from the surface of the last prism; but this arrangement is not applicable to the automatic spectroscope, because the last surface of its last prism is perpendicular to the axis of the eye-telescope. An equivalent arrangement may, however, be made by using one of the forms of prism whose sections are given in the figures, as the last prism of an automatic spectroscope. The light to be analyzed having passed through the rest of the train of prisms, enters the last prism, which is immovable, through the side AB , and emerges through CD , the light which is brought to a focus in the centre of the field emerging perpendicularly to CD . Let the light from the cross-wires be made parallel by a lens, and let PQ be the direction of the light from the centre of the cross. In fig. 1 PQ is perpendicular to the axis of the eye-telescope, in fig. 2 PQ is inclined at an angle of 120° , and in fig. 3 at an angle of 60° to this direction. $PQRS$ gives the course of the light from the centre of the micrometer-cross; and RS is perpendicular to the side CD ; and therefore the centre of the cross will be distinctly seen in the centre of the field at the same time as the spectrum. The form of prism to be used depends upon the position in which it is desired to place the micrometer. The diameter of the pencil of light from the micrometer is taken in the figures as one third of that through the prisms; and the latter is taken as the unit of length in the following dimensions for each prism.

Fig. 1.

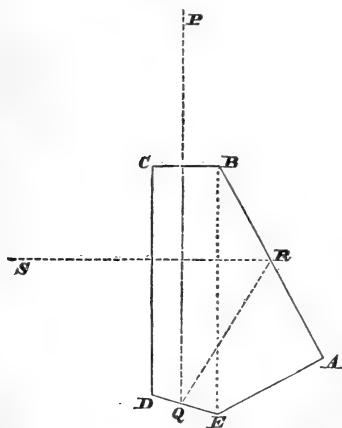
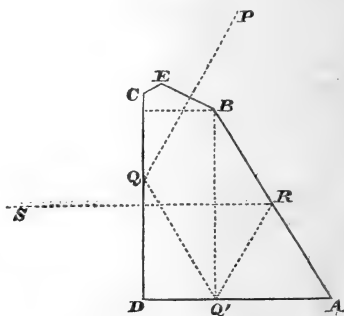


Fig. 2.



* Communicated by the Author.

Fig. 1. Angles :— $B=120^\circ$; $C=90^\circ$; $D=105^\circ$. $BE=\frac{4}{3}$, and is parallel to CD ; $BC=\frac{1}{3}$; $BA=\frac{2}{\sqrt{3}}=1.15$; AE is unpolished.

The angle between BA and $DE=45^\circ$.

Fig. 2. Angles :— $A=60^\circ$; $B=150^\circ$; $D=90^\circ$. $BE=\frac{1}{3}$; the perpendicular from B on $CD=\frac{1}{3}$; the perpendicular from B on $AD=1$; DC is not less than 1; CE is unpolished.

The angle between BE and $DC=60^\circ$.

Fig. 3.

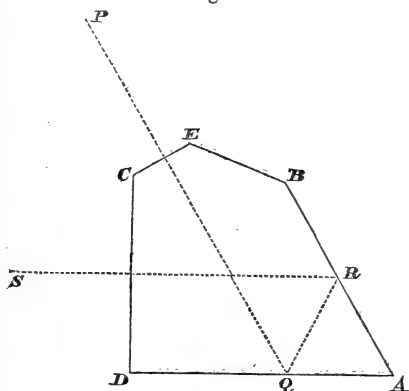


Fig. 3. Angles :— $A=60^\circ$; $C=120^\circ$; $D=90^\circ$. $CE=\frac{1}{3}$; $CD=1$; $DA=\frac{7}{3\sqrt{3}}=1.35$; $AB=\frac{2}{\sqrt{3}}=1.15$; BE is unpolished.

The angle between AB and $CE=90^\circ$.

In the scale to which the figures are drawn the unit is one inch. If desirable the sides DE in fig. 1, and DA in figs. 2 and 3, may be silvered, as they are only used for reflection.

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XXXVII. "*In the Beginning.*"—I. *Mass and Position.* By PLINY EARLE CHASE, *Professor of Mathematics in Haverford College*.*

IN tracing the influence of undulations, propagated with the velocity of light, upon cosmical aggregation and dissociation, I have shown that the ratio of the limiting velocity of incipient dissociation to the limiting velocity of incipient aggre-

* Communicated by the Author.

gation is $1 : \pi^*$. While I have questioned the propriety of accepting the nebular hypothesis in the sense in which it is commonly understood, I have no hesitation in yielding it the qualified acceptance proposed by Herschel †, and in recognizing its value for representing the results of tendencies to equilibrium between centripetal and centrifugal forces.

The position of Saturn at the mean centre of inertia of the solar system, its low density, and its nebulous rings, would seem likely, *à priori*, to furnish indications of important relationships between equilibrating forces. Such indications are actually found, both in planetary masses and in planetary positions. For if we examine the masses of the supraasteroidal planets, we find the following accordances:—

1. Neptune is to Saturn, as the time of describing radius, *in direct fall* to the centre, is to the time of circular orbital revolution, or as $1 : \sqrt{32}$.

2. Neptune and Saturn are in inverse ratio to their times of orbital revolution. They would therefore have equal moments of inertia, near their lower nebular or nucleal radii ‡.

3. Uranus is to Saturn, as the time of describing radius, *in a circular orbit*, is to the time of orbital revolution, or as $1 : 2\pi$.

4. Uranus and Saturn are in inverse ratio to the square roots of the times of rotation of Jovian nuclei, the radii of which correspond with their respective primitive Jovicentric vector radii §, or inversely as the velocities of the nucleal rotations. They would therefore have equal momenta, with reference to Jupiter, in the primitive nucleal arrangement.

5. Jupiter and Saturn are in the inverse ratio to the times of nucleal rotation for nucleal radii corresponding to their respective vector radii.

6. Consequently Jupiter and Saturn had equal moments of inertia, if they were once parts of the same nebulous belt ||.

7. Jupiter is to the aggregate planetary mass, as circular orbital velocity is to parabolic perihelion velocity, or as $1 : \sqrt{2}$.

8. If the aggregate planetary mass were collected at Jupiter's linear centre of oscillation, the centre of gravity of the system would be at Sun's surface.

9. Jupiter and the residual supraasteroidal mass are so

* Phil. Mag. [IV.] vol. 1. p. 251.

† Outlines of Astronomy, Sect. 871.

‡ The upper nebular or vector radii vary as the $\frac{2}{3}$ powers of the orbital times; the lower nebular or nucleal radii, as the $\frac{1}{2}$ powers of the times.

§ See Proc. Amer. Phil. Soc. Sept. 17, 1875.

|| When I first announced this relation (Proc. Amer. Phil. Soc. vol. viii. p. 141) I was not aware that it had been communicated to the American Association by Professor Stephen Alexander in 1857. See his 'Statement and Exposition' (Smithsonian Contributions, 280), p. 38.

related as to give Jupiter an inertia-moment, at a centre of spherical gyration, equivalent to that of the remaining mass at the corresponding spherical surface.

10. Saturn and the Neptune-Uranian belt are so related as to give equal momenta, in reference to a nodal division midway between the lower nebular, or nucleal, radii of Saturn and Uranus.

11. Jupiter and Sun are so related as to balance each other, in a linear pendulum, of which the geometrical mean planetary radius vector represents a centre of oscillation, and Sun's surface represents both a centre of suspension and a fulcrum.

Late estimates of mass* give the following values, Sun being the unit :—

Jupiter	·0009543269	Uranus	·0000454545
Saturn	·0002855837	Neptune	·0000507614

Aggregate Planetary mass ·0013421925.

On the basis of these values the following Table is constructed :—

	A.	B.	(A-B) ÷ A.
1. $h = \Psi \times \sqrt{32}$	·0002856	·0002872	—·006
2. $h = \Psi \times t \div t'$	·0002856	·0002839	+·006
3. $h = \delta \times 2\pi$	·0002856	·0002856	—·001
4. $h = \delta \times 48 \cdot 16 \div 7 \cdot 572$	·0002856	·0002891	—·012
5. $h = \mathcal{A} \times t \div t'$	·0002856	·0002839	+·006
6. $h = \mathcal{A} \times d_1^2 \div d^2$	·0002856	·0002839	+·006
7. $\mathcal{A} = \text{Pl.} \div \sqrt{2}$	·0009543	·0009491	+·005
8. $\mathcal{A} = \odot \div (\sqrt{2} \times \frac{2}{3} \times 1117 \cdot 9)$...	·0009543	·0009488	+·006
9. $\mathcal{A} = h \delta \Psi \div 4$	·0009543	·0009545	·000
10. $h = \delta \Psi \times 21 \cdot 15 \div 7 \cdot 14$	·0002856	·0002850	+·002
11. $\mathcal{A} = \odot \div (\frac{2}{3} \times 696 \cdot 54)$	·0009543	·0009571	—·003

The fourth accordance is computed as follows. The rotation-radii, or the radii of solar nuclei which would rotate synchronously with the respective planets, are, for Neptune 49·0168, Uranus 35·006, Saturn 20·726, Jupiter 13·154, Sun's present radius being the unit. Stockwell has found† that the mean perihelion longitudes of Jupiter and Uranus differ by exactly 180°, while the mean node longitudes of Jupiter and Saturn also differ by 180°. Therefore, if the two outer two-planet belts were compared before their hypothetical rupture, the rotation-radii, when referred to Jupiter, would be :—

$$\text{Saturn} = 20 \cdot 726 - 13 \cdot 154 = 7 \cdot 572 ;$$

$$\text{Uranus} = 35 \cdot 006 + 13 \cdot 154 = 48 \cdot 16.$$

* Proc. Amer. Phil. Soc. 1875, p. 3.

† Smithsonian Contributions, 232, p. xiv.

In the eighth and eleventh accordances, Earth's mean radius vector being 214·86 solar radii, $5\cdot2028 \times 214\cdot86 = 1117\cdot87$; and the geometrical-mean radius vector

$$(\varnothing \times \varnothing \times \bullet \times \varnothing \times \varnothing \times \varnothing \times \varnothing \times \Psi)_{\frac{1}{8}} = 696\cdot54.$$

The tenth accordance is found by first taking the mean of the rotation-radii of Saturn and Uranus

$$\left(\frac{20\cdot7258 + 35\cdot0062}{2} = 27\cdot866 \right),$$

and regarding it as a node of reference for the rotation-radii of Saturn and Neptune

$$(27\cdot866 - 20\cdot7258 = 7\cdot1402; 49\cdot0168 - 27\cdot866 = 21\cdot1508).$$

The relationships of position seem to be primarily dependent upon the properties of elastic media. In such media, as the distances from a centre of force increase in arithmetical progression, the densities decrease, in geometrical progression if the central force is constant, in harmonic progression if the central force varies inversely as the square of the distance. In the earliest stages of nucleal aggregation, when particles are subjected to nearly equal impulses from every direction, but with a slight preponderance towards special nucleal centres, the variations from constancy of force may be so slight as to introduce a geometrical progression; and if those tendencies occur when the contest between the aggregating and dissociating forces is the strongest, the ratio of such progression should be $1 : \pi$. If we compare the vector radii of the aphelion planets in each of the aphelion or supraasteroidal two-planet belts, with the vector radii of the perihelion planets in each of the perihelion or infraasteroidal belts, we find the following coincidences—Saturn, with its nebulous ring and its position of mean planetary inertia, being the starting-point for the π -geometrical series as well as for the correlative determinations of planetary mass. The tabular unit is Sun's radius.

	A.		B.	(A-B) \div B.
$h \times \pi^1$	6438·75	Ψ mean	6453·06	—·0022
$h \times \pi^0$	2049·51	h "	2049·51	
$h \times \pi^{-1}$	652·38	\ast "	652·38	
$h \times \pi^{-2}$	207·66	\oplus perihelion	207·58	+·0004
$h \times \pi^{-3}$	66·10	\varnothing "	68·48	—·0361
$\oplus \times \pi^{-1}$	68·39	\varnothing "	68·48	—·0013

These several accordances of mass and position are brought into simple correlation with my planetary pendulums* by the nucleal action of the velocity of light, at the geometrical-mean

* Phil. Mag. [IV.] vol. 1. p. 250.

planetary radius vector, which corresponds to planetary velocity at the mean perihelion centre of gravity of Sun and Jupiter; for Jupiter's mean excentricity being $\cdot04316^*$, the mean perihelion centre of gravity is at

$$\cdot95684 \times 1117\cdot87 \div 1047\cdot879 = 1\cdot0208.$$

The time of planetary revolution is

$$1 \text{ year} \div (214\cdot86 \div 1\cdot0208)^{\frac{3}{2}} = 10333\cdot8 \text{ seconds};$$

the velocity is therefore $2\pi r \times 1\cdot0208 \div 10333\cdot8$ per second. The velocity of light is $214\cdot86 r \div 497\cdot83$, which is $695\cdot4$ times the theoretical equatorial velocity of the nucleus, or the equatorial velocity of a nuclear atmosphere extending to $695\cdot4 r$. This differs by less than $\frac{1}{6}$ of 1 per cent. from the mean planetary radius vector.

Philadelphia, November 1875.

XXXVIII. *Notices respecting New Books.*

Physical Geography, or the Terraqueous Globe and its Phenomena. Illustrated with 125 wood engravings, frontispiece, and 12 maps. By WILLIAM DESBOROUGH COOLEY. London: Dulau and Co., 1876 (8vo, pp. 429).

“GEOGRAPHY, as the word imports, is a delineation or description of the earth. In its widest sense this comprehends not only the delineation of the form of its continents and seas, its rivers and mountains, but their physical condition, climates and products, and their appropriation by communities of men”†. Accordingly it admits of division into three fairly distinct branches, viz. Mathematical, Descriptive, and Physical Geography. The first might be classed as a branch of Astronomy; the second, if its relations to History are put on one side, is little more than a description in words of the facts that are presented to the eye in a good set of maps. The third, Physical Geography, can scarcely be considered as a separate science; it is rather a delineation of facts based upon a knowledge of all natural sciences; it passes in review whatever is known respecting the form, internal heat, electro-magnetic tension of our planet, the volcanic reaction of its interior on its solid crust, the phenomena of its aerial and watery oceans, the distribution of the various forms of organic life over its surface, and, so far as they can be made out, the causes whose action in the past have determined the present condition of the surface, and the distribution of organic forms upon it. It is plain that a subject embracing so many topics, many of them with scarcely any mutual connexion that can be traced, admits easily of division in several

* Stockwell, *op. cit.* p. 38.

† Outlines of Astronomy, c. iv., by Sir J. Herschel.

different ways at the will of the writer. Mr. Cooley proceeds as follows:—He excludes Descriptive Geography, and all that relates to organic life, and to the history of previous conditions of the surface; at least, if notice is taken of these subjects, they come in as episodes. By way of introduction he gives a sketch of Mathematical Geography, and so much Astronomy as is needed for its exposition, and then takes so much of Physical Geography as relates to what is frequently called the “inorganic life” of the earth. To use his own words, Physical Geography “is the department of science which embraces the course of physics reigning on the earth’s surface over land, sea, and air.” It will be seen from this that our author takes a view of the limits of his subject different from that taken by most writers. No objection can be raised to his doing this in a subject so extensive that an accurate acquaintance with both the excluded and included parts can rarely be acquired by one man. He also differs from other writers in another way. They commonly assume their readers to be acquainted with the physical principles which are exemplified in terrestrial phenomena. Mr. Cooley, however, thinks it better to include an account of these principles in the course of his work. The consequence is that their discussion is not sufficiently full to meet the wants of learners, and is unnecessary for others; besides, the need of brevity in the exposition of these incidental matters has apparently led, in some cases, to what is, at best, inexact statement.

The twelve maps with which the work is illustrated are, as might be expected, maps of Isothermal, Isobaric, Cotidal, Isogonal, and Isoclinal lines, of regular winds and rains, and of Oceanic Currents. They are all well drawn; and the six of Isothermal and Isobaric lines are very elegant maps, and very distinct, though on so small a scale that they are printed with a good margin on an octavo page. It is to be regretted that maps of this kind are not printed in such a manner as to indicate approximately the degree of confidence that can be placed on the several lines. In Europe and elsewhere they are fairly trustworthy; but in some cases the data must be very insufficient, and in others a bold distinct line is continued through districts where for 1000 miles at a stretch not a single observation has ever been made; *e.g.* in the map of Isothermal lines for July the southern Isotherm of 60° passes through Southern and Western Australia about midway between the tracks of Mr. Eyre and Colonel Warburton, and the southern Isotherm of 80° goes through the middle of New Guinea nearly along its length.

A work like the present embraces a wide range of facts; and of course there is a proportionately great risk of errors occurring here and there. We have compared a considerable number of Mr. Cooley’s statements with those of other writers; and we see no reason to doubt their general accuracy. But we think his book can hardly be regarded as a work of authority. We have noted several points in which his statements are either inexact, or liable to be questioned. We will give a few instances.

On p. 15, and again on p. 21, we are informed that "a second of space is about the sixtieth part of the smallest point distinctly visible to the naked eye." This must depend on the eye and the light; we doubt whether, at a distance of 290 feet, any eye could see a circle an inch in diameter *as a circle*; but an ordinary eye could see it in a good light at a considerably greater distance, and Saturn with an apparent diameter of about 15" is distinctly visible on a clear night. On p. 26 we read that "the force of rotation of a globe like the earth, nearly 8000 miles in diameter, and spinning round at the rate of more than 17 miles a minute, is quite sufficient to render the position of its axis incapable of change." By position we assume the author to mean position within the body of the earth, because in space its position, or rather direction, undergoes change from precession, in virtue of which it describes a cone round a line at right angles to the plane of the ecliptic. The reason why the line within the earth round which it rotates never varies much from a mean position is neither the size of the globe nor its angular velocity, but its shape. It is the bulge at the equator which confines the axis of rotation to nearly one position within the earth. On p. 45 we meet with the following statement:—"The most remarkable instance of abnormal gravitation occurs at the southern base of the Himalaya Mountains. In consequence of it, we are told, the sea at Karachi, near the mouth of the Indus, is 514 feet above the sea at Cape Comorin, the line of level being raised to that extent by the attraction of the mountains." The authority for this statement, we presume, is a paper by the late Archdeacon Pratt, of which an abstract is given in vol. ix. pp. 597-9 of the Proceedings of the Royal Society. Subsequently, however, the Archdeacon revised his calculation and brought out as a result that, in consequence of the attraction of the Himalayas and the deficiency of matter in the Southern Ocean, the sea would be 116 + 448 or 564 feet higher at Karachi than at Cape Comorin; but then he adds that, owing to counterbalancing causes, the change in the sea-level is insensible ('Figure of the Earth,' 4th ed. pp. 233, 201). On p. 73 our author expresses himself so incautiously as to lead an uninformed reader to suppose that the image of a point seen by reflection is formed on the reflecting surface. On p. 106 we come on the following strange statement. Commenting on an opinion of M. Pouillet, that the temperature of space is much above the absolute zero, and that this is attributable to radiation from the stars, our author observes, "He seems to have miscalculated through forgetting that a high temperature can be brought about only by a rise in the scale, and not by a multiplication of low temperatures. If the heat communicated by each star be 32°, then the heat of 10,000 stars will be no greater than that of so many icebergs."

On the whole Mr. Cooley has written an interesting and, in some respects, valuable work; but in the study of it the reader will have to be on his guard, as the statements, though generally trustworthy, are sometimes open to exception. Matters of specu-

lation (such as the doubts raised by the author as to the original fluidity of the earth) speak for themselves; and the reader may be expected to take discussions of such questions for what they are worth.

XXXIX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 249.]

January 6, 1876.—Joseph Dalton Hooker, C.B., President, in the Chair.

THE following papers were read:—

“On the Action of Light on Tellurium and Selenium.”
By Prof. W. G. Adams, F.R.S.

Two platinum wires were attached to the ends of a small bar of tellurium, about 1 inch in length, by heating the wires to a bright red heat and bringing them suddenly into contact, one with each end of the bar. The platinum wires melted a small portion of the tellurium, and became imbedded in it. The resistance of the bar and wires was about half an ohm. The tellurium was placed in a box, and its resistance balanced; then it was exposed to the light of a paraffin-lamp, just as in the experiments with selenium. At first the light seemed to have no effect; but in consequence of the heat from the lamp, the resistance of the tellurium was increased.

On placing a rectangular vessel of water between the lamp and the tellurium and then exposing as before, there was no change of resistance in the tellurium, showing that if there was any diminution in the resistance due to the action of light it was entirely balanced by the increase in the resistance due to heating by the current and by the residual portion of the radiant heat.

On replacing the rectangular vessel of water by a beaker of water, so as to focus the light of the lamp on the tellurium, there was found on exposure to be a *diminution in the resistance of the tellurium*, which gradually increased until the galvanometer-needle was deflected through 30 divisions of the scale. The needle was then brought back to zero by altering one of the slide-resistances in the circuit. The agreement between the amounts of the change of resistance in several successive experiments, with intervals between them, was very close. Thus, in three experiments on October 8th,

The change in the first experiment was 124 millims. of wire,

“ “ second “ “ 120 “ “

“ “ third “ “ 122 “ “

showing a diminution in the resistance of the tellurium amounting to about one thousandth part of its whole resistance on exposure to the light of the paraffin-lamp.

On repeating the experiments with the tellurium which had not

been exposed to light for seven days, the tellurium was found to be much more sensitive. When exposed at a distance of about half a metre, as before, to the paraffin-lamp the needle was gradually deflected through 30 divisions of the scale, showing that the resistance of the tellurium was now *diminished* as much without interposing the beaker of water as it had previously been when the beaker was interposed. On making no change whatever, except placing the beaker of water between the lamp and the tellurium, so as to focus the light on the tellurium, the deflection of the needle gradually increased to 80 and then more slowly to 100 divisions. On balancing this deflection, it was found that, to bring the needle to zero, it was necessary to diminish the slide-resistance by 400 millims. Thus the diminution produced in the resistance by exposure to the light of the paraffin-lamp was $\frac{1}{300}$ th of the whole resistance of the tellurium.

On exposing the selenium bar used in my experiments to the direct rays of the same paraffin-lamp at the distance of 1 metre, the resistance of the selenium was diminished by one fifth of its whole resistance.

From the above experiment we see that at the distance of half a metre (that is, with light of four times the intensity) the change of resistance in the tellurium under the same conditions is only $\frac{1}{1000}$ th part of its whole resistance.

On exposing the selenium to a constant source of light at different distances, the change in the resistance of the selenium on exposure for 10 seconds (as measured by the swing of the galvanometer-needle) is almost exactly inversely as the distance, *i. e.* directly as the square root of the illuminating power. This law is true whether the source of light be 1 candle or an Argand lamp whose illuminating power is equal to 16 candles.

Taking the mean of a number of experiments, all of which agreed pretty well together, the deflections at the several distances were :—

	At $\frac{1}{2}$ metre.	At $\frac{1}{2}$ metre.	At 1 metre.	At 2 metres.
With Argand lamp	170	83	39
„ candle.....	..	41	18	8
„ candle.....	82	39	18	8

Another series of experiments with a candle and Argand lamp (when the illuminating power of the lamp was equal to 12 candles), both at the distance of 1 metre, gave the following results :—

With the candle the deflection was 19 in 10 seconds.

„ Argand lamp „ 66 „

The ratio of the deflections is very nearly 1 to $3\frac{1}{2}$.

These experiments clearly show that the change in the resistance of the selenium is *directly as the square root of the illuminating power*.

[Correction.—In my former paper on this subject (Phil. Mag. Feb. 1876), on page 156, line 17, omit the word “opposing,” and line 23, for “which opposes” read “in the same direction as”; also on page 159, line 23, for “which opposes a” read “in the same direction as the”; to the end of line 25 add “but in the opposite direction.”]

“On the Refraction of Sound by the Atmosphere.” By Prof. Osborne Reynolds, Owens College, Manchester.

This paper may be said to consist of two divisions. The first contains an account of some experiments and observations undertaken with a view to ascertain how far the refraction of sound caused by the upward variation of temperature may be the cause of the difference in the distances to which sounds of the same intensity may be heard at different times.

Some rockets, capable of rising 1000 feet and then exploding a cartridge containing 12 oz. of powder, having been procured, an effort was made to compare the distance at which the rockets could be heard with that at which a gun, firing $\frac{1}{4}$ lb. of powder and making a louder report than the rockets, could be heard under the same conditions of the atmosphere. In the first instance the rockets and the gun were fired from a spot in Suffolk around which the country is tolerably flat, observers being stationed at different distances. Owing, however, to the effect of the wind and the time required for the observers to proceed to the distant stations, these experiments were not successful in establishing the comparative merits of the gun and the rockets. They were, however, important as showing that on hot calm days in July the reports of the rockets never failed to be distinctly audible at distances of 4 and 5 miles, although the sun at the time was shining with full force on the ground and rendering the air near the surface so heterogeneous that distant objects seen through it appeared to wave about and twinkle.

The next attempt was made during a cruise on the east coast. After three weeks cold and windy weather, the 19th of August was a fine day; and some experiments were made in Lynn Deep, which revealed a very extraordinary state of the atmosphere as regards the transmission of sound. A party rowed away from the yacht in one of her boats, it having been arranged beforehand that either a rocket or a large pistol was to be fired from the yacht when signalled for; also that when those on the yacht heard those in the boat call they should answer. The boat proceeded to a distance of 5 miles, until those on the yacht had completely lost sight of it; but all the time the calls from the boat were distinctly heard by those on the yacht, although after they had lost sight of the boat they ceased to answer the calls. On the boat also not only were the reports of the pistol and rockets distinctly heard, but every answer from the yacht was heard plainly. The last came after an interval of 35 seconds, which gave the distance $3\frac{1}{2}$ miles. Nor was this all; but guns, and on one occasion the barking of a dog, on the shore 8 miles distant were distinctly heard, as were also the paddles of a steamer 15 miles distant.

The day was perfectly calm, there was no wind, the sky was quite clear, and the sun was shining with great power—conditions which have been described as most favourable to the stoppage of the sound by the heterogeneity of the atmosphere, and which may also be described as most favourable for great upward refraction. On this day, however, it was observed that all the time distant objects

loomed considerably, *i. e.* appeared lifted. This showed that the air was colder near the surface of the sea than it was above. It is to this circumstance that the extraordinary distances to which sounds were heard on this day is supposed to be due. The diminution in the temperature of the air being downwards, the sound, instead of being lifted as it usually is, was brought down, and thus intensified at the surface of the water, which, being perfectly smooth, was thus converted into a sort of whispering-gallery.

The report of the pistol and the sound of the voices were attended with echoes, but not so the reports of the rockets; and it is suggested that these so-called echoes may be found only to attend sounds having a greater intensity in one direction than in another.

The second part of the paper refers to a phenomenon noticed by Arago in his report of the celebrated experiments on the velocity of sound made on the nights of the 21st and 22nd of June, 1822.

It was then found that, although the guns fired at Monthéry could be distinctly heard at Villejuif (11 miles distant), those fired at Villejuif could not be heard at Monthéry without great attention, and at times (particularly on the second night) they were not heard at all—although on both nights the wind was blowing from Villejuif to Monthéry, the speed of the wind, which was very light, being about 1 foot per second. No explanation of this phenomenon was offered by the observers, although it was much commented on. And on the second night the gun at Villejuif, which on the previous night had been pointed upward, was brought down in the hope that this might improve its audibility (this step, however, was found to render matters worse than before).

From this lowering of the gun at Villejuif it seemed as though there was probably some difference in the conditions under which the guns at the two stations were placed, as if that at Villejuif was fired from a level, while that at Monthéry might be fired over a parapet. An inspection of the district confirmed this view; for Villejuif is on a low flat hill, while Monthéry is on the top of a steep cone; and not only is it 80 feet above Villejuif, but it is surmounted by the mound of an old castle, which is supported by a vertical wall towards Villejuif and surrounded by a low rampart. Hence it is suggested that in all probability the advantage of the gun at Monthéry was due to its being fired over this parapet, while that at Villejuif was fired from the level ground.

The fact that the wind blowing from Villejuif did not reverse this advantage, suggested the possibility that at night, when the diminution of temperature is downward, a light wind may not produce the same effect upon sound as when the diminution of temperature is upward, as it generally is during the day.

To ascertain if this is the case, some observations were made on some calm nights in May and June of the present year, from which it was found:—

(1) That when the sky was cloudy and there was no dew, the sound of an electric bell 1 foot above the grass could always be heard further with the wind than against it; but

(2) that when the sky was clear and there was a heavy dew, the

sound could invariably be heard as far against a light wind as with it, and in some cases much further. On one occasion, when the temperature at 1 foot above the grass was 38° and at 8 feet 47° , and the speed of the wind was 1 foot per second at 5 feet above the grass, the bell was heard 440 yards against the wind and only 270 with it.

Since, therefore, on the nights of the experiments at Villejuif and Montlhéry it is stated that the sky was clear, that there was dew, and the temperature recorded at the two stations shows the diminution to have been downwards, it is argued that the effect of the wind to render the sound less audible at Villejuif was completely balanced by the downward refraction of temperature.

Another phenomenon recorded by Arago is, that while the reports of the guns at Montlhéry as heard at that station were attended with prolonged echoes, this was not the case with those at Villejuif. It is thought that this difference is sufficiently accounted for by the fact that while Montlhéry is surrounded by high hills with precipitous or wooded sides, which must produce echoes, the country in front of Villejuif is very flat and has not a tree upon it for miles.

In concluding the paper reference is made to the Appendix to the last Report of the American Lighthouse Board, in which Dr. Henry, the Chairman, gives an account of his experiments, extending over thirty years, and the conclusions to which they have led him, both of which are in favour of the apparent stoppage of the sound being due to refraction.

GEOLOGICAL SOCIETY.

[Continued from p. 170.]

Jan. 19, 1876.—John Evans, Esq., F.R.S., President, in the Chair.

The following communications were read:—

1. "On some Unicellular Algæ parasitic within Silurian and Tertiary Corals, with a notice of their presence in *Calceola sandalina* and other fossils." By Professor P. Martin Duncan, F.R.S., V.P.G.S., &c.

2. "How Anglesey became an Island." By Prof. A. C. Ramsay, LL.D., F.R.S., V.P.G.S.

The author described, and illustrated by sections drawn to scale, the contours of the island of Anglesey and the adjacent parts of Carnarvonshire, and noticed that the whole island may be regarded as a gritty undulating plain, the higher parts of which attain an average elevation of from 200 to 300 feet above the sea-level. Similar conditions are presented by the country for some miles on the other side of the straits; and in both the general trend of the valleys is north-east and south-west. The rock surfaces, when bare, show glacial striæ running generally in a direction 30° to 40° west of south.

The author indicated that the great upheavals of the crust of the earth forming mountains took place long before the commencement of the Glacial epoch, and that ordinary agents of denudation had

ample time for the formation in mountain-regions of deep valleys, down which, during the Glacial epoch, glaciers would take their course. He noticed the evidence of this local glaciation furnished by the striation of the Welsh mountains, from which he inferred that these mountains as a whole were not overridden by a great ice-sheet coming from the north, and he described the course of the glaciers flowing from the north-west slopes of Snowdonia as being in the directions west, north-west, and north. These glaciers, however, did not reach the region now occupied by the Menai Straits, but spread out in broad fans on the north-western slopes of the hills now overlooking the Straits—a fact indicated by the directions of the glacial striæ in these parts. Anglesey, therefore, was not glaciated by ice-masses coming from Snowdonia; and as the striations on that island point directly towards the mountains of Cumberland, the author inferred that these markings were produced by a great ice-flow coming from that region, reinforced probably by ice-streams from the north of Scotland, and which were large and powerful enough to prevent the glaciers of Llanberis and Nant-francon from encroaching on the territory of Anglesey.

The author described the rocks bordering the Straits as consisting of nearly horizontal Carboniferous strata, which, from appearances, must once have filled the whole of the region now occupied by the straits. He considered that the softer shaly, sandy, and marly beds, remains of some of which are still to be seen on the coast, were swept away by the action of the great glacier coming from the north-east, forming a valley now occupied by the sea; and in support of this view he cited the valley of Malldraeth Marsh, running across Anglesey, parallel to that of the Menai Straits, about 4 miles to the north-west, which a very slight change in conditions would convert into a fjord, differing from the Straits only in being closed at the north-east end.

Feb. 2, 1876.—John Evans, Esq., F.R.S., President, in the Chair.

The following communications were read:—

1. "Evidence of a carnivorous Reptile (*Cynodracon major*, Ow.) about the size of a Lion, with remarks thereon." By Prof. Owen, C.B., F.R.S., F.G.S., &c.

2. "On the Occurrence of the Genus *Astrocrinites* (Austin) in the Scotch Carboniferous Limestone Series, with the Description of a New Species (*A. ? Benniei*), and Remarks on the Genus." By R. Etheridge, Esq., Jun., F.G.S.

3. "On the Genus *Merycochoerus* (Family Oreodontidæ), with descriptions of two new species." By G. T. Bettany Esq., B.A., B.Sc.

February 23, 1876.—Professor P. Martin Duncan .B., F.R.S.,
President, in the Chair.

The following communications were read:—

1. "On the Greenstones of Western Cornwall." By John Arthur Phillips, Esq., F.G.S., F.C.S.

In this paper the author brought forward evidence to show that the so-called "greenstones" of Penzance really belong chiefly to the following three classes:—

a. Gabbros or Dolerites, in which the originally constituent minerals are either to a great extent unchanged, or, sometimes, almost entirely represented by pseudomorphic forms.

b. Killas, or ordinary clay-slates.

c. Highly basic hornblendic rocks, exhibiting a tendency to break into thin plates; these under the microscope present the appearance of metamorphosed slates.

Slaty rocks of a character intermediate between *b* and *c* also occur.

In the Cape-Cornwall district the "greenstones" are chiefly hornblendic slates, sometimes with veins or bands of garnet, magnetite, or axinite. The rocks near the Gurnard's Head are almost identical with those of Mount's Bay. The crystalline pyroxenic rocks and metamorphic slates of the St.-Ives district exactly resemble those of Penzance. The greenstones between St. Erth and St. Stephen's are probably altered ash-beds or hardened hornblendic slates; unlike the hornblendic and augitic rocks of the other districts, they do not occur in the immediate vicinity of granite; but elvan courses are always found near them. The percentage of silica in the two series of rocks is nearly constant: the hornblende slates contain about 10 per cent. less silica than the crystalline pyroxenic rocks; and there is an excess of iron oxides to nearly the same extent, their composition in other respects being very similar. The Killas is an acidic rock of essentially different chemical composition.

2. "On Columnar, Fissile, and Spheroidal Structure." By the Rev. T. G. Bonney, M.A., F.G.S.

Some of the above structures have comparatively recently been discussed by Mr. Mallet and Professor J. Thomson. Both these authors agree in attributing columnar structure to contraction due to loss of heat while cooling, but differ in their explanation of cross jointing and spheroidal structure. In this paper it is sought to show that the principle proved by Mr. Mallet to be the explanation of the columnar structure is capable of a wider application. After a brief notice of some instances of columnar structure, the author described cases of a fissile structure seen in certain igneous rocks (especially in the Auvergne phonolites), closely resembling true cleavage, and often mistaken for it—also the tabular jointing of rocks, a peculiar form of this where most of the segments are of a flattened convexo-concave form, spheroidal structure, and cup-and-ball structure. He showed by examples that Prof. Thomson's explanation of spheroidal structure was inadequate, and gave reasons for considering all these structures to be due to contraction. He also discussed more particularly the cup-and-ball structure, giving reasons for thinking that the spheroidal and the horizontal fissures were often to some extent independent of each other.

March 8, 1876.—Professor P. Martin Duncan, M.B., F.R.S., President, in the Chair.

The following communications were read:—

1. "On the influence of various substances in accelerating the precipitation of Clay suspended in water." By Wm. Ramsay, Esq., Principal Assistant in Glasgow University Laboratory.

The author referring to the fact that clay when suspended in water in excessively minute particles, settles more rapidly when the water contains salts in solution, noticed the opinions expressed by previous writers on the subject, and gave the results of experiments made by him, from which it would appear that the rapidity of precipitation is proportionate to the amount of heat absorbed by the salts in process of solution. By another series of experiments he found that the fluidity of the respective solutions had apparently no influence on the rapidity of deposition of the clay. He also found that clay is deposited less quickly in acid solutions than in solutions of salts, and more rapidly in a solution of caustic soda than in one of caustic potash. In solutions of common salt of different strengths he found that clay settled in the inverse order of their specific gravities. From all these results the author is inclined to attribute the varying rapidity of the settling of clay suspended in saline solutions to the varying absorption of heat by the solutions. When water containing suspended clay was heated, the rapidity of the settling of the clay was proportionate to the heat of the water. The author suggests that the increased rapidity of settlement may be due to the greater amplitude of vibration of the molecules of water when heated; the vibrations being performed in equal times, particles descending at right angles to the plane of vibration will experience less resistance from the molecules of water.

A note by Prof. Ramsay, briefly indicating some of the geological bearings of these results, was appended to the paper.

2. "On some Fossiliferous Cambrian Shales near Carnarvon." By J. E. Marr, Esq.,

The shales described by the author extend from about three miles S.W. of Carnarvon to Bangor, running nearly parallel to the Menai Straits. They are faulted against Lower Cambrian to the east, and disappear against a dyke on the west. The shales vary from greyish black to bluish black in colour, and are generally sandy and micaceous, but in places chiefly clayey. Fossils were obtained from three places on the banks of the Seiont—namely, near Point Seiont (where the beds are concretionary in structure), along the old tramway from Carnarvon to Wanttles, and near Peblig Bridge. The first-named locality is richest in fossils; and here there is a greenstone dyke, parallel to the bedding of the rock, and altering the shales for a distance of about four yards from the edge of the dyke. The fossils seem to indicate that the deposit belongs to the upper part of the Arenig group.

3. "On the occurrence of the Rhætic Beds near Leicester." By W. J. Harrison, Esq., F.G.S., Curator of the Town Museum, Leicester.

The sections described by the author are shown in brick-pits in the Spinney Hills, forming the eastern boundary of the town of Leicester, and in the Crown Hill on the eastern side of a valley excavated by the Willow Brook. In the latter locality they are capped by Lower Lias. They have a slight dip to the south-east. The brick-pits show a thickness of about 30 feet of Rhætic beds

above the Triassic red marl, to which their stratification is parallel. The lowest bed is a light-coloured sandy marl about 17 feet thick, traversed by three or four courses of harder, whiter stone, and containing crystals of selenite, pseudomorphs of salt, and numerous small fish-scales. A single insect-wing was obtained from it. This bed extends across the valley of the Willow Brook, and forms the base of Crown Hill. Above it comes the Bone-bed, from 2 to 3 inches thick, containing numerous small teeth, bones, and scales of fishes and Saurians, including large vertebræ of *Ichthyosaurus*, ribs probably of *Plesiosaurus*, and some bones of Labyrinthodont character. Two species of *Axinus* also occur. The Bone-bed is followed by about $2\frac{1}{2}$ feet of coarse black shales, overlain by a very thin band of hard reddish sandstone, with casts of *Axinus*, and this by about 2 feet of finely laminated black shales containing *Cardium rhæticum*, *Avicula contorta*, and a Starfish (*Ophiolepis Damesii*). Above these come about 5 feet of shales with sandy partings, the lower foot rather dark and containing *Avicula contorta*, *Cardium rhæticum*, *Ostrea liassica*, and a new *Pholidophorus*; the remainder light-coloured, but with the same shells. The topmost bed in the section is a band of nodular limestone 6 inches thick. The same sequence is observed in Crown Hill. There are indications of the existence of a second nodular limestone and of beds of light-coloured clay and sand, but obscured by drift, in which, however, blocks of limestone occur with *Monotis decussata* and *Anoplophora musculoïdes*. The author indicates other localities where traces of the Rhætic beds are to be seen, and states that wherever the true junction of the Trias and Lias is exposed the Rhætics appear to be invariably present. The paper also included some particulars with regard to borings in the Trias near Leicester.

4. "Hæmatite in the Silurians." By J. D. Kendall, Esq., F.G.S.

The author referred to a former paper, in which he showed that the direction of the hæmatite deposits in the Carboniferous Limestone of Cumberland and Lancashire is parallel to that of the meridional divisional planes, or nearly north and south; while the deposits in the Silurians are in two directions, some parallel to one set of divisional planes and some to the other. In the present paper he describes a deposit of hæmatite at Water Blean, in the parish of Millom in Cumberland, in Coniston Limestone, which appears to be altogether unlike those referred to in his former paper. The Silurians here are all conformable, with a strike about 65° N.E. and S.W. and a dip of about 80° to N.W.; but their order is inverted. The hæmatite occurs in the Coniston Limestone in the form of short veins, varying in width from a few inches to 9 feet, running in the direction of the strike, and having the same dip as the limestone, their deposition having taken place along the bed-joints of the rock. The author accounts for this difference in the deposits by the fact that in the Coniston Limestone at Water Blean the bed-joints are much more persistent than the divisional planes, which are very irregular and not at all so strong and open as the bed-joints.

XL. *Intelligence and Miscellaneous Articles.*ON THE SPECTRA OF NITROGEN AND THE ALKALI-METALS IN
GEISSLER TUBES. BY G. SALET.

M. SCHUSTER made public, in 1872, the important fact that nitrogen, heated in a Geissler tube with metallic sodium, no longer gives its characteristic grooved spectrum. He described the bright lines obtained in this case, and attributed them to pure nitrogen—the band spectrum being, in his opinion, that of an oxide of nitrogen, a compound which is destroyed by the alkali-metal. Later, doubts arose concerning the validity of these conclusions; for, on the experiments being repeated, the grooves were indeed seen to disappear after the action of the sodium, but they were replaced by various spectra, of which not one really belongs to nitrogen, so that after purification this gas could no longer be detected by the prismatic analysis. I have also remarked that the chemical compound which is in reality formed by the action of oxygen upon nitrogen is the peroxide, a very stable body and one the spectrum of which does not at all coincide with that of which we have to account for the appearance.

My present purpose is, to demonstrate (1) that the grooved spectrum can be produced with nitrogen heated in contact with sodium, (2) that the disappearance of the spectrum of nitrogen is due to the disappearance of the nitrogen itself, it being entirely absorbed by the sodium under the influence of the electric effluvium, and (3) that the spectrum described by M. Schuster is very probably to be attributed to the vapour of the alkali-metal.

1. An account of the series of experiments which have led me to these conclusions would be too long to give here; I will only cite the most decisive. I had a tube of hard glass blown by M. Alvergnyat, stopped at both ends, 12 centims. in length and 2 centims. in diameter. Two aluminium electrodes were arranged at one end of the tube, separated by a distance of about a centimètre; to the other extremity a tubule with a bulb was soldered. Into the bulb a little piece of sodium was introduced; and the tubule was then cemented to the mercury air-pump. A vacuum having been produced, the sodium was heated; it swelled and boiled for a long time, losing hydrogen; it then ceased to boil, and at a higher temperature slowly volatilized. The apparatus was now severed from the air-pump by a stroke with a pipe, and the shining globule of liquid sodium was conveyed into the experiment-tube. After cooling, the bulk was separated, and the tube was cemented directly to the pump. Exhaustion was then recommenced, the sodium volatilized, care being taken that the condensation of the metallic vapour should only be produced in that half of the tube which carried no electrodes; and nitrogen was admitted pure and dry. I again exhausted, to the amount of three fourths of the nitrogen, taking care each time to volatilize the alkali-metal, and finally closed the apparatus, leaving in it a pressure of about 5 millims. I could then melt the globules, cause them to unite,

and volatilize them again a dozen times in contact with the same mass of gas, without the appearance of the spark which exploded between the electrodes being modified in the slightest degree. I made use of the Holtz machine, or an induction-coil with the addition of a Leyden jar: the space between the poles was rosy violet, and gave the grooved spectrum with the utmost purity. When the disruptive spark of the Holtz machine is employed, the jet of rosy-violet flame, which gives the grooved spectrum, is instantaneous; we can assure ourselves of this by a very simple method: the luminous jet is viewed through the glass wheel of the machine, on which little black points are traced with ink. Now these points appeared perfectly distinct, even when the handle made sixty turns in a minute; we can hence infer that the discharge does not last a *thirty-thousandth* of a second.

2. By volatilization the sodium can be readily brought into the vicinity of the electrodes. It presents itself then, as always, under the aspect of very pure silvery white shining globules; but if the tube be brought into play, the portions submitted to the action of the jet of flame tarnish immediately: the metallic appearance completely disappears; and the surface of the sodium becomes brownish black. At the same time are seen the changes in the appearance of the electric jet induced by greater rarefaction. If we renew the surface of the sodium, the action continues and the nitrogen-spectrum entirely disappears; the light is yellowish, and due for the most part to the sodium. I say, *for the most part*; for, in these conditions of an almost absolute vacuum some traces of foreign lines are always perceived in the spectrum, attributable to impurities of the electrodes and of the inner surface of the glass. In this case the tube, being cold, does not permit the spark of the Leyden jar to pass.

I have made a direct experiment to manifest this absorption of nitrogen by sodium under the influence of electricity. I had a tube constructed like the preceding, but carrying a truncated barometer; and I introduced into it nitrogen at the pressure of 27 millims. The absorption of the gas was sufficiently sensible for the eye to be able to follow the rise of the column of mercury. After some minutes, the sodium-surface having been renewed twice, no difference could be perceived between the levels of the mercury in the two branches of the manometer.

I have moreover endeavoured to trace the chemical characters of this absorption: I broke a tube and separately treated with water a portion of the sodium which remained bright and a portion altered by electricity. Into both solutions I poured some of Nessler's reagent (iodargyrate of potassium with an excess of soda). One of the liquors became very yellow; it was that into which the altered sodium had been thrown. The other did not change its appearance; there had therefore been formed under the influence of the electricity some nitride of sodium, decomposable by water with production of ammonia. This body is only generated at a higher temperature than a red-heat, like the nitride of magnesium, or is even not directly produced at any degree of heat, like am-

monia. I intend to prepare and analyze it further.—*Comptes Rendus de l'Académie des Sciences*, vol. lxxxii. pp. 223–226.

The lines described by M. Schuster* have not been found again by MM. Stearn and Wüllner, who, after verifying the disappearance of the band spectrum, saw the well-known spectrum appear of the oxide of carbon or acetylene. Schuster's tubes more carefully prepared gave me, after a certain time, the bright lines of hydrogen only. What, then, was the origin of the lines described in the memoir of 1872? It is permissible to attribute them to the vapour of sodium.

In fact, we remark that the published numbers were not got by direct measurement, but refer to the nitrogen lines of Plücker's spectrum which appear to coincide with the lines observed; and these, moreover, are not the most characteristic. Now, if sodium be heated in a Geissler tube, or in an apparatus like that described in §1, and the spark be made to pass, a bright greenish yellow light is produced, the spectrum of which is composed of the following lines:—

Spectrum of sodium.

615.5 double	515.3
589.2	„ (D) 498.3
568.7	„ 467

These numbers are very near those of M. Schuster; only the line 498.3 does not figure in his Plate, it is replaced by 489.4. Conversely, we find in that Plate three extreme lines—628.8, 421.4, and 418.4, which I have not been able to produce, nor are they indicated by MM. Thalén and Lecoq de Boisbaudran. A curious thing, these lines nearly coincide with the characteristic lines of rubidium (629.6, 421.6, and 420.2). I would not by any means say that this metal was present in M. Schuster's sodium, especially with the uncertainty of the wave-lengths; but in any case it must very readily give a spectrum in a Geissler tube; for potassium offers much better facilities for this kind of experiments than sodium. It furnishes, without difficulty, a very pure spectrum, of which the principal lines are the following:—

Spectrum of potassium.

583	535.3	511	404.4
580	533.5	509	
578.3	532		

It must be possible, employing the same method, but under slightly different conditions of experiment, to produce not only the secondary spectra of the alkali-metals, as we have done in the present researches, but also their primary spectra, for the interesting discovery of which we are indebted to Messrs. Roscoe and Schuster.—*Comptes Rendus de l'Académie des Sciences*, vol. lxxxii. pp. 274, 275.

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ON CERTAIN REMARKABLE POINTS IN MAGNETS. BY R. BLONDLOT.

If a very short magnetic needle, the centre of gravity of which is supported, be brought near the surface of a magnet, the direction

* Pogg. *Ann.* vol. cxlvii. p. 106.

of the needle is seen to vary at the same time as the coordinates of its centre of rotation. Among these directions there are some remarkable ones to which we would call attention—namely, those normal to the surface of the magnet. For the sake of brevity, we will give to the points to which they correspond the name of *orthogonal points*.

To take an example, let us consider a magnetized bar having the form of a rectangular parallelepiped; on each half of the bar we shall find five orthogonal points:—one on the terminal face, at the point where the magnetic axis meets it; and one on each of the four lateral faces, situated on the right line joining the centres of the short sides of this face, the last four points being symmetrical, two and two.

Around each of the points we are considering, the directions are such that, reckoning from the surface of the magnet inwards, they are always convergent in any plane drawn through the normal.

A first property of orthogonal points is the following:—

If a small magnetic body be placed on an orthogonal point, more mechanical work will be requisite, in order to remove the small body thence to infinity, than if it had been placed on any other neighbouring point of the surface of the magnet. In other terms, the orthogonal point presents a *maximum* of the work above-mentioned.

The demonstration of this theorem is very simple.

With respect to terrestrial magnetism, the orthogonal points of the surface of the earth are what are somewhat improperly called the magnetic poles. Halley and Hansteen believed in the existence of four of these poles; Gauss and Duperrey admit only two, or, more precisely, two polar regions. Parry, John Ross, James Ross, Dumont d'Urville, and Wilke were able to determine approximately their positions.

On a given line, also, points possessing the same property of maximum of work of removal to infinity are found; they are those for which the magnetic action is normal to the curve.

We will remark that all these *maxima* presented by orthogonal points might be replaced by *minima* for surfaces presenting suitable ratios of curvature with the surfaces of equal potential. A simple change of sign of the work of sliding conducts immediately to this result.

A second curious property is the following:—

The positions of spontaneous equilibrium of a small magnetic body with respect to a magnet are precisely the orthogonal points. This proposition flows immediately from the fact that the positions of equilibrium of a point situated on a polished surface are those for which the forces acting on the point produce no tangential component.

This is confirmed by the following experiment:—A particle of iron is fixed on a flat cork, which is then put to float upon water. If a magnet be brought quite close to the surface of the water, with its lower face horizontal, the cork will be seen to move until the iron particle which it carries is placed precisely under the orthogonal point. If the magnet be placed in any manner whatever in space, the final situation of the iron will indicate the point of con-

tact of the surface of equal potential which touches the surface of the water.—*Comptes Rendus de l'Académie des Sciences*, vol. lxxxii. pp. 454, 455.

ON THE METALLIC REDUCTIONS PRODUCED IN CAPILLARY SPACES.

BY M. BECQUEREL.

When a cracked tube containing a concentrated metallic solution (of nitrate of copper or chloride of cobalt for example) is dipped into a solution of monosulphide of sodium, if the crack is not sufficiently narrow, diffusion is produced, giving rise to the production of metallic sulphide in the solution of copper or cobalt; this sulphide forms a coat adhering pretty strongly to the glass; and then by degrees a deposit of bright metal is seen to form in the molecular space between the coat of sulphide and the glass, on the side of the metallic solution.

Similar effects are produced by applying to the crack, outside the tube, a strip of paper covered with a layer of freshly precipitated sulphide, fixing it to the surface by means of a wire wound round it. Cobalt, copper, platinum, &c. have thus been obtained in the metallic state; the first was attracted by the magnet.

Effects like these might indeed take place in organic nature in the case of rupture of tissues or vessels. Suppose, for example, that a vessel which traverses a muscle is ruptured at any point; blood is immediately diffused into the muscle; and a coagulum is formed, which is in contact on one side with the blood, on the other side with the liquid that moistens the muscle. An electrocapillary action will then take place resembling the foregoing, giving rise to a reducing or oxidizing action, according to the nature of the liquid with which the blood is in contact when it coagulates. The products then formed may cooperate in closing the aperture. I merely indicate the forces which come into play, in the impossibility of knowing what products are formed.

Doubtless similar effects are produced in wounds covered with a plaster coated with a substance of a healing nature.

The communication finishes with a recital of the chemical reactions produced in capillary spaces with the cooperation of a voltaic couple of two liquids, adjunct but forming part of the apparatus. The couple is formed of a cracked tube containing a solution of monosulphide of sodium, in which is immersed a slip of platinum; and it is introduced into a test-tube containing a metallic solution. Thus arranged, the two-liquid couple operates in consequence of the reaction of the two liquids in the fissure; two currents result, travelling in the same direction—one the current in question, the other the electrocapillary current described in my previous memoirs. On the other hand, the platinum wire wound round the tube touching the crack being the negative electrode at which the reduction is effected, it follows that the two actions are added together, as can readily be explained; and thus the amount of reduction is doubled.

It is to be remarked that electrocapillary apparatus formed of cracked tubes act only so far as the two liquids, penetrating the

crack, come into contact, which does not take place when the glass is too thick. The Note contains some details on the operation of this mixed electrocapillary apparatus, with a constant current, without the intervention of an oxidable metal as in the couples of voltaic piles.—*Comptes Rendus de l'Académie des Sciences*, vol. lxxxii. pp. 354–356.

ON THE CHEMICAL ACTIONS PRODUCED BY THE DISCHARGES
FROM AN INDUCTION-APPARATUS. BY M. BECQUEREL.

The memoir commences by recalling the experiments of Wollaston on the decomposition of water by the electricity from the ordinary electrical machine, as well as those made by Faraday for the purpose of rendering sensible the decomposition of salts, when he sought solely to make evident the presence of acids and bases by aid of test-papers.

With a Ruhmkorff induction-apparatus, which gives a much more rapid succession of sparks than the ordinary electrical machine, more marked effects are obtained. The arrangement I have adopted to exhibit them is the following:—Taking a plate of gutta percha on which was placed a small slip of platinum in communication with the positive pole of the apparatus by means of a metallic stem, I applied to this slip of platinum a strip of paper moistened with a metallic solution (copper or silver), and then to the paper the point of a platinum wire connected with the negative pole. It was not long before the metal was seen depositing around the point in adherent layers. On a slip of platinum being placed between the paper and the point of this metal, it likewise became coated with a thin layer of metal. Submitting to experiment successively various solutions, the apparatus operating with only two chromic-acid couples, and sometimes with four, I thus obtained the reduction of copper, nickel, cobalt, iron, lead, bismuth, antimony, zinc, cadmium, silver, gold, and platinum.

I likewise applied myself to the formation of amalgams by aid of the same apparatus, following the method which Davy employed, with the pile, to obtain the amalgams of potassium, sodium, and other metals. On a slip of platinum which was brought into contact with the positive pole of the apparatus I placed a piece of caustic potass slightly moistened, and introduced into a small cavity in its surface a drop of mercury in contact with the point of a platinum wire or the negative pole of the induction-apparatus; after a few moments the globule changed into a pasty amalgam in which I perceived some crystals of this compound.

The copper-amalgam was obtained by operating with a mixture of a solution of nitrate of copper and nitrate of mercury with which the band of paper applied on the platinum slip was moistened; in the same way were produced the amalgams of aluminium, magnesium, and other metals. I conceive that the discharges of an induction-apparatus, when proceeding from electricity of high tension and taking place in rapid succession, are capable of producing powerful chemical effects.—*Comptes Rendus de l'Académie des Sciences*, vol. lxxxii. pp. 353, 354.

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XLI. *On a Method of Measuring very small Intervals of Time.*
By MR. ROBERT SABINE*.

THE method I am going to describe of determining any very small interval of time which elapses between two successive mechanical actions, I believe to be trustworthy when proper precautions are used. It is based upon the fact that a charged Leyden jar or other form of accumulator† can only be discharged at a certain definite rate through a given circuit.

It is common knowledge that if a telegraph cable or accumulator have an electrostatic capacity of f farads, and the resistance to leakage or discharge between its two sides be R ohms, the time (t seconds) required for any initial charge of the potential P to diminish to a remainder at the potential p will be

$$t = fR \log_{\epsilon} \frac{P}{p} \text{ seconds,}$$

assuming that all the electricity which we have to deal with resides upon the coatings and that it is free to be discharged. This proportion, otherwise expressed, is commonly used for finding the insulation resistances of submarine cables when their capacities are known and the initial charge is allowed to leak or discharge through the dielectric alone during a given number of minutes.

In employing this method for the measurement of time, it is

Communicated by the Author.

† Commonly called a "condenser."

Phil. Mag. S. 5. Vol. 1. No. 5. May 1876.

2 A

necessary to know exactly the value of the resistance; and as this could not be depended upon if the discharge took place through the dielectric, a known wire resistance, r , has to be inserted between the two sides of the accumulator, so that the time during which the potential P of the charge falls to p is

$$t = \frac{f \log_{\epsilon} \frac{P}{p}}{\frac{r+R}{rR}} \text{ seconds.}$$

The internal or insulation resistance R of the accumulator being made so great in comparison with r that it may practically be regarded as infinite*, the time is then expressed by

$$t = fr \log_{\epsilon} \frac{P}{p} \text{ seconds.}$$

Assuming that the excursion of the light-point of a mirror-galvanometer is proportional to the quantity of electricity suddenly discharged through its coil, if the excursions are respectively C and c , the time is

$$t = fr \log_{\epsilon} \frac{C}{c} \text{ seconds.}$$

The only values therefore required to be known are the capacity (f) of the accumulator, and the resistance (r) of the discharging wire. The measurement is independent of the constant of sensibility of the galvanometer and of the electromotive force of the battery, neither of which, however, must vary during an observation.

I have employed this system to determine the intervals of time which elapse

(1) Between the successive interruptions of two circuits, and

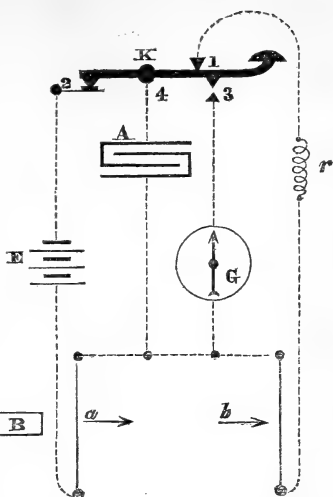
(2) Between making and breaking a single circuit.

The interval of time to be measured is, of course, always that during which the accumulator is allowed to partially empty itself through the discharging resistance.

When the time to be measured is that which elapses between the successive interruptions of two circuits, the apparatus is arranged in principle as is shown in fig. 1. The two circuits

* With a mica accumulator of $\frac{1}{3}$ microfarad capacity, the discharging resistance, r , necessary for determining intervals of time between $\frac{1}{100}$ and $\frac{1}{10}$ of a second is under 1 megohm. The internal resistance of such an accumulator is considerably over 200,000 megohms, so that practically it may be assumed to be in all cases infinite in comparison with r .

Fig. 1.



which have to be broken are *a* and *b*,—first *a*, then *b*. In the figure, *a* and *b* are supposed to represent, by way of illustration, two thin wires stretched at different distances from the muzzle, *B*, of a gun in such a way that the shot passes through them in the direction of the arrows. The battery *E* has so little resistance* in comparison with that of the discharging wire *r*, that the potential of the accumulator, *A*, remains practically unaltered when *r* is made infinite.

A key of peculiar construction is convenient for observing the initial charge. It consists of a lever (*K*) turning on pivots at 4. When in its position of rest (as shown in the figure) the rigid contact, 1, at the top in front, and the spring contact, 2, underneath at the back, are both in connexion with the lever. When the knob is depressed the rigid contact 1 is first interrupted; an instant afterwards the spring contact, 2, is interrupted; and lastly the front contact, 3, is made. This arrangement ensures the removal of the discharging resistance *r*, before the interruption of the battery, by the key.

Depressing for a moment the knob of the key, the resistance and battery are therefore successively interrupted, and the instantaneous discharge from the accumulator is read off by the excursion of the galvanometer-needle at *G*. This gives the value *C*.

Letting the key resume its position of rest and recharge the accumulator, the wires *a* and *b* are in turn interrupted by firing a shot through them. The interruption of *a* puts the battery out of action; and the charge which is in the accumulator at the moment commences to flow out through the discharging wire *r*. This flow is stopped as soon as the shot reaches and breaks *b*. The observer, as soon as he hears the report, depresses the key and reads the excursion (*c*) of the needle due to the remainder of the charge.

The accuracy of this method depends upon the conditions (1) that the difference of potential between the two sides of the accumulator is not altered practically by its poles being

* A single cell of Daniell's battery of large surface is sufficient.

joined by the discharging wire, and (2) that the time which elapses between the interruption of the circuit *b* and the pressing down of the key is not sufficient to allow of any loss through the dielectric. Further on I will show how nearly these conditions may be secured.

It is, of course, necessary to have every part of the apparatus as well insulated as possible; and it is desirable that the discharging resistance and a shunt across the galvanometer should be so adjusted that all excursions have about the same value. The following are a series of observations made by firing a bullet from a small breech-loading pocket-pistol through two wires set up (as in fig. 1) from one to four feet apart, the muzzle being close to the first wire. The accumulator was insulated with mica and shellac; its capacity was $\frac{1}{3}$ microfarad ($f = 0.333 \times 10^{-6}$ farad); its leakage in fifteen minutes was only 1 per cent., and therefore its insulation resistance about 250,000 megohms. The discharging wire had a resistance of 100,000 ohms. The constant (*fr*) was therefore = 0.0333.

Experiment.	Range, <i>d.</i>	Excursion of light.		Time of flight, calculated $\left(fr \log_e \frac{C}{c}\right)$, <i>t.</i>	Calculated velocity of shot, $\frac{d}{\bar{t}}$
		C.	c.		
	feet.	divisions.	divisions.	second.	feet per sec.
1.	1	300	280	0.0023	436
2.	1	300	281	0.0022	457
3.	1	300	280	0.0023	436
4.	2	300	251	0.0059	340
5.	2	300	256	0.0053	379
6.	2	300	260	0.0048	422
7.	2	300	259	0.0049	408
8.	3	299	240	0.0073	408
9.	3	299	245	0.0067	450
10.	3	299	250	0.0061	496
11.	3	299	256	0.0052	576
12.	3	299	246	0.0065	460
13.	4	299	230	0.0087	458
14.	4	299	229	0.0089	451
15.	4	299	225	0.0095	422
16.	4	299	231	0.0086	467

A glance at these results shows that the separate observations were not more different from the mean value than could be well accounted for by the inequality of the charges of powder in the cartridges.

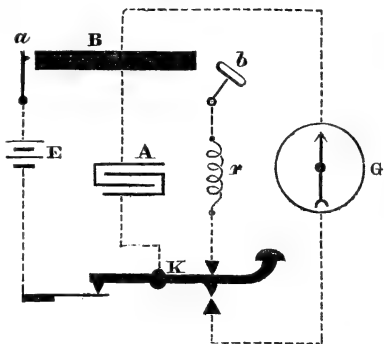
When the time to be determined is that which elapses between making and breaking a circuit, I have found that the apparatus may conveniently be arranged in principle as is shown in the sketch, fig. 2.

The poles of the battery are not required to be connected by

the discharging resistance r while charging the accumulator ; and therefore a very much lower value of r may be employed than in using the preceding method, without fear of error. By way of illustration, B is a

Fig. 2.

block of metal or anvil against which the contact, a , is pressed in order to charge the accumulator, A, by the battery E. The light hammer, b (weighing about 1 ounce), is then suddenly struck against B and allowed immediately to rebound off again. During the interval which elapses between the contact by b and its interruption, or what may be termed the electrical duration of the blow, the accumulator leaks through r ; and the remainder is measured on the galvanometer (G) by depressing the key (K) as before. The same accumulator was used in these experiments as in the previous ones.



Excursion of light-point.		r .	Duration of flow, calculated $\left(fr \log_e \frac{C}{\sigma} \right)$, t .
Initial charge, C.	Remainder, c.		
308	265	ohms.	second.
308	264	1000	0.000050
308	269	1000	0.000051
308	265	1000	0.000045
307	261	1000	0.000050
306	260	1000	0.000054
306	262	1000	0.000054
307	267	1000	0.000052
			0.000047
		Mean ...	0.000050

It now remains to be shown :—

1. That the results obtained by this method agree amongst themselves, and
2. That the intervals of time calculated by the formula agree with the observed time.

The observations made with the hammer and anvil (fig. 2) were sufficiently uniform to suggest that, by discharging the accumulator gradually by a series of blows of as nearly equal force as possible, the time of each blow might be regarded as

an equal instalment of the whole time during which the discharge through the resistance-wire took place.

The following series of observations, with their calculated results, amply proves that this view was correct :—

Number of blows given by hammer, <i>n</i> .	Excursion of the light-point.		Resistance of discharging wire, <i>r</i> .	Calculated time	
	<i>C</i> .	<i>c</i> .		of discharge, <i>t</i> .	of each blow, $\frac{t}{n}$
10	311	109	ohms. 1500	second. 0·000524	second. 0·000052
9	311	121	1500	0·000471	0·000052
8	311	139	1500	0·000402	0·000050
7	311	105	1000	0·000362	0·000052
6	311	120	1000	0·000317	0·000053
5	310	138	1000	0·000269	0·000054
4	310	128	700	0·000206	0·000051
3	310	125	500	0·000151	0·000050
2	309	118	300	0·000096	0·000048
1	309	145	200	0·000050	0·000050
				Mean ...	0·000051

In 1872 I commenced a series of experiments with the view of comparing the results calculated by this method with the time given by some form of chronoscope. The difficulty I met with, however, was to find a chronoscope of sufficient sensibility to give a contact or series of contacts of known duration. For this purpose the late Sir C. Wheatstone, with the generous aid he always gave to any physical inquiry, placed in my hands an apparatus which had been very ingeniously designed and constructed for him some years before by Mr. Stroh for a different purpose; and with this apparatus, furnished with suitable electric contacts, I made several attempts to check the correctness of the formula*.

This apparatus consisted of a disk of metal with a heavy rim, set in rotation by the force of a spring which was arrested by striking against an anvil, whilst a pointer turning with the disk struck a blow against the end of a small lever at some point of the revolution determined by its position. The duration of the contact was arranged to last from the instant when the impelling spring struck the anvil until the pointer on the disk struck the lever; and as it was assumed (and found) that the velocity at any point of a single revolution was practically uniform, the placing of the pointer enabled an interval of very small duration to be ensured. The results which I obtained, however, with this apparatus, although highly interesting,

* I was much indebted to the industrious aid which Mr. J. Rymer Jones rendered me in these observations.

were not concordant, there appearing to be a variable and rather considerable interval between the closing of the contact and the motion of any electricity in the circuit. This behaviour, which had induced Sir C. Wheatstone to lay the apparatus aside, induced me to regard it also as not sufficiently exact for the purpose for which he had suggested that I might be able to use it.

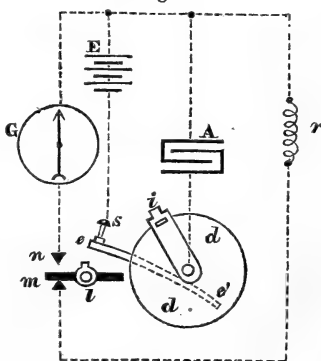
Much occupation prevented me taking up this subject again until recently, when I recommenced by making a more careful study of this little contact-apparatus, and found the cause of its irregularity to be vibration due to the sudden stopping of the impelling spring. I therefore had it reconstructed so that each part was placed upon a separate base, and found that it then fulfilled all that could be desired of it.

The periphery of the rotating disk is divided into 500 equal parts; and as the finger or pointer is furnished with a *nonius* by which $\frac{1}{10}$ of a division may be accurately read, or $\frac{1}{5000}$ of the periphery, it follows that when the disk makes two revolutions per second, the finger may be placed to give a blow at any desired interval between 0.0001 and 0.5 second.

The force of the spring is adjusted as nearly as can be to give the requisite initial speed of rotation by observing the engraved divisions of the disk, which become visible in succession under the fibre of a small telescope when illuminated by the spark of an induction-coil, the primary circuit of which is opened and closed by a half-seconds' pendulum.

The way in which I have employed this apparatus for controlling the above method of electrically determining very small intervals of time is as follows:—The body of the disk *d* and spring are connected to one side of the mica accumulator, *A*, fig. 3. The spring *e* *e'*, whilst trigged, rests in contact with a point *s*, connected with one side of the battery *E*. The remaining sides of the accumulator and battery are connected together. In this position, therefore, the battery charges the accumulator. Contact *m* is joined through the adjustable wire resistance *r*, and contact *n* through the galvanometer *G*, with the point of junction between battery and accumulator. On releasing the disk the spring *e*, leaving *s*, breaks the battery-circuit, and,

Fig. 3.



coming into contact with an anvil in electrical connexion with the support of the lever l , allows the charge of the accumulator to leak partially away through m and r until the pointer, i , rotating with the disk, strikes l from m to n , thereby stopping further discharge through r , and putting the accumulator to the galvanometer, which indicates the remaining electricity, c .

In the experiments which follow, the object was to compare the relation of the excursions $\left(\frac{C}{c}\right)$ as observed with the same relation calculated by the aid of the formula, on the assumption that the time-intervals as indicated by the index of the pointer on the disk were correct. The same accumulator was used as in the previous experiments. The battery consisted of two LeClanché cells. The galvanometer-coil was shunted by a wire whose multiplying-constant was $=3$ in reading the excursions due to the initial charges C .

Interval of time by index of disk.	Discharging resistance (r).	Excursions of needle observed.		$\frac{C}{c}$.	
		C .	c .	Observed.	Calculated.
second.	ohms.	divisions.	divisions.		
0.0002	500	232×3	204	3.41	3.32
0.0003	700	232×3	206	3.38	3.62
0.0004	1000	233×3	226	3.09	3.32
0.0005	1200	233×3	214	3.27	3.49
0.0006	1600	233×3	228	3.06	3.08
0.0007	1700	233×3	210	3.33	3.44
0.0008	2000	234×3	216	3.25	3.32
0.0009	2300	234×3	226	3.11	3.23
0.001	2500	234×3	218	3.22	3.32
0.002	5000	234×3	224	3.13	3.32
0.003	7000	234×3	216	3.25	3.62
0.004	10000	235×3	208	3.39	3.32
0.005	12000	235×3	200	3.52	3.49
0.006	14000	235×3	198	3.56	3.62
0.007	17000	235×3	206	3.42	3.44
0.008	20000	236×3	210	3.37	3.32
0.009	23000	235×3	212	3.33	3.23
0.01	25000	237×3	215	3.31	3.32
0.02	50000	236×3	213	3.33	3.32
0.03	70000	233×3	196	3.57	3.62
0.04	100000	231×3	208	3.34	3.32
0.05	130000	232×3	217	3.20	3.17
0.06	160000	236×3	230	3.08	3.08
0.07	180000	235×3	219	3.22	3.21
0.08	200000	235×3	210	3.36	3.32
0.09	230000	235×3	218	3.24	3.23
0.1	250000	234×3	210	3.34	3.32

It is evident from this Table that the agreement between the

intervals adjusted mechanically and those calculated by means of electrical leakage is very close, and that the formula $(t = fr \log_e \frac{C}{c})$ is not only applicable for slow discharges through very high resistances, but equally so for quick discharges through low resistances.

These two series of test-experiments I think show conclusively that this system affords results which, through a wide range of time-intervals, are comparable with each other—and, further, that within a small percentage these calculated intervals are correct.

In the method in which two circuits are broken, the discharging resistance is kept in circuit whilst the accumulator is being charged, on the assumption that the battery-resistance is so small in comparison with it that the potential of its poles is not thereby altered. This is theoretically evident; and the following observations of the instantaneous discharge when the battery-poles were connected by resistances of various values show that, until the discharging wire is reduced in resistance much below that which is required to be used in the method, no appreciable error is introduced.

Resistance between poles of battery.	Discharge.
Infinite	277
100000 ohms	277
10000 „	277
1000 „	276

It is also assumed in both ways of working, that the time which elapses between the interruption of the discharging-wire and the closing of the galvanometer-circuit is insufficient to allow of leakage through the dielectric of the accumulator. The following experiment was made for the purpose of ascertaining the actual leakage of the accumulator for various periods of insulation :—

Instantaneous discharge	=275
Discharge after 1 minute	=275
„ 15 minutes	=272
„ 30 „	=269

It is evident, therefore, that, were even fifteen minutes allowed to elapse before taking the reading, the error through this delay would be only about 1 per cent. There is nothing, however, to prevent the reading being taken within a second or two.

In employing this system I have found that there are certain precautions which it is necessary to keep in view. In the first place, it is desirable, in taking the readings of the excursions of the needle, to so adjust the resistance-wire that the value of c is not too nearly the same as that of the initial charge C . When they are nearly of the same value, a very slight error of observation makes a material error in the result, because the *difference* of their logarithms may increase or decrease much more rapidly than the numbers themselves. On the other hand, the proportion between C and c should not be too great; in other words, the accumulator should not be too empty when its reduced charge is observed. The electricity which flows from a battery into an accumulator (assumed to be perfectly insulated) is employed in two ways: (1) it *suddenly* charges the coatings inductively; and (2) it *gradually* polarizes or charges the dielectric material. When the charged coatings are connected through a small resistance, the coatings are first discharged suddenly, and the "diselectrification" or discharge of the dielectric (which commences at the same instant of time) goes on gradually. We should therefore be prepared to find a small quantity of electricity still in the accumulator long after the coatings have been discharged. This is more observable when the discharge takes place suddenly than when it takes place gradually (that is, through a high resistance), because in the latter case the diselectrification-current, which is very small in comparison with the discharge-current, has time to creep out with it unobserved.

This, however, has an effect only when the accumulator becomes very empty and the readings of c are so small as to come within its disturbing influence. By selecting the discharging-resistance of such a value that the measured remainder from the accumulator is never less than one third or one fourth of the initial charge, all fear of error from this cause may be dismissed.

There are numerous applications in which the determination of time-intervals of very small duration may be found of practical value; and I think that this system may be depended upon.

In conclusion I must acknowledge the valuable assistance which I have had, both in arranging the apparatus and in making the observations, from Mr. M'Eniry.

25 Cumberland Terrace, N.W.,
February 1876.

XLII. *On Electromachines with Disks of Ebonite.*

By Dr. L. BLEEKRODE*.

[Plate III.]

THE name "electromachine" is in Germany specially applied to electrical machines in which no friction is used to generate electricity, but in its place a small quantity is introduced into the apparatus to act by induction on metallic points fixed before rotating surfaces, and this action is reinforced by the peculiar arrangement of the machine. This principle was first employed by W. Holtz, at Berlin, in 1865, in the construction which bears his name.

The object of the present communication† is to place before the reader the results of an investigation of this machine, which has led to the substitution of ebonite plates for glass ones‡. I had recourse to this material because the atmospheric moisture in Holland (as it may also, perhaps, in England) seriously hinders the activity of the apparatus, and in many cases causes it not to work at all. But not only did this change of material prove very useful; I also found the machine, constructed on my system, to possess some very remarkable properties, different from those observed when glass disks are used, and worth being more generally known§.

I. *The Electromachine of the First Species with ebonite disks.*

Description of the apparatus.—The electrical machine of the first species, as it is commonly called, consists of one fixed disk, before which another somewhat less in diameter rotates rapidly at a very short distance and passes along a set of horizontal points fixed on conductors. I may suppose the construction to be sufficiently known, as it is the same that was employed by Borchhardt of Berlin; it presents the simplest form, and has spread widely over the continent. I have augmented the surface of the paper armatures by extending them almost over two opposite quadrants of the fixed disk, to the horizontal apertures; and I found it advantageous to cover the adhering paper points with tinfoil.

Remarks on ebonite.—It has been already observed that the peculiar character of my machine is the substitution of ebonite for glass, so that it is wholly composed of that sub-

* Communicated by the Author.

† A more detailed description of the apparatus with illustrations and the results obtained, is published in Poggendorff's *Annalen*, No. 10, 1875.

‡ My first machine with ebonite disks dates from the year 1873.

§ The machine here described (and probably also a double machine) will be seen working in the Loan exhibition at South-Kensington Museum in London, having been sent by the makers.

stance ; and, as far as I could consult different periodicals, I have found no mention of a true Holtz machine with disks of ebonite. Ebonite (a mechanical mixture of india-rubber and sulphur) has many advantages for this purpose, which glass can but rarely offer. It is capable of developing much more electricity, and readily becomes electric ; it is much less hygroscopic, and, especially with regard to the boring of the apertures in the disk, is cheaper. Here may also be added that no part of the apparatus is liable to be broken. Yet unfavourable properties must be noticed, which are well known to experimenters with this substance. It tends slowly to alter its composition, especially if exposed to the solar rays or to the action of ozone ; the sulphur is separated and transformed into sulphuric acid, and the insulating properties are destroyed. But it may be observed here that some qualities of ebonite are the result of great negligence ; sometimes a bad kind of raw material enters into its manufacture ; so that very different sorts are found in commerce. When a demand shall have been created for a quality of ebonite specially suitable for electrical machines, it may be expected that more attention will be paid to its production, so that it will be possible by careful selection to obtain an excellent material.

I have now for more than two years used the same disks ; and when their action was not energetic, I soon restored it by rubbing them with magnesia and petroleum *. When the weather was very damp this proved very efficient ; if exposed to moderate heat, it was sufficient to rub the disks with the hands to render the machine active as soon as they were put in the proper position. These precautions are easily practised, because the construction is very simple and there is no chance of breaking any part. Often, as I have observed, the alteration of the ebonite is but superficial ; in this case the insulating property is recovered when the surface has been again polished. At all events this change will not readily ensue when the machine is kept, after use, out of direct daylight ; and it is also very convenient in this case to place the disks on a horizontal surface ; this will remove the curved superficies which appear, when their diameter does not exceed 2 millims.

Exciting the machine.—The machine with disks of ebonite has the remarkable property that, although its action is as energetic as that obtained with glass disks, it is never excited when the inclined or diametral conductor is not present †. It

* This liquid is a very excellent developer of the electric qualities of ebonite.

† This name is applied to the conductor which is fixed in the axis before the rotating disk, opposite to the paper armature, and inclined at an angle of nearly 70° to the horizon.

is absolutely necessary ; and even when powerful sparks are passing between the conductors, the electric manifestation disappears immediately as soon as the inclined conductor is taken away : the disks seem to have entirely lost their charge ; and even in the dark not the slightest visible trace of electricity can be detected in the apparatus. The action is completely resumed when its inclined conductor is again brought to the armatures, even at a distance of 2 or 3 centims. The apparatus may be excited also by placing for some moments a piece of rubbed ebonite behind the rotating disk opposite to the points of the inclined conductor. Much electricity, attracted by induction, flows on this surface from the points ; and this soon causes a stream of sparks to pass between the horizontal conductors. It must be observed that the excitation is only obtained when these conductors are not in contact ; yet this is necessary when glass disks are used. I tried, but in vain, to put the apparatus in action without the inclined conductor, or with it when the horizontal conductors were brought close together.

Results obtained with the machine.—In the machine above described I have used disks of the greatest diameter hitherto employed. That of the fixed disk is 58 centims., and of the rotating one 55 centims. The condensers in connexion with the horizontal conductors are Leyden jars, with a covered surface of 1 square decim. Without them, when the negative conductor ends in a spheroid (9 centims. diameter) and the positive in a sphere (2 centims. diameter), a continuous stream of sparks passes at a distance of 4 centims. At a greater distance a beautiful sheaf appears between the conductors, consisting of a stem of 2 or 3 centims. with a bundle of thin violet threads which continue to radiate towards the opposite conductor, even at a distance of 17 centims., neutralizing the negative electricity. When the condensers are connected, energetic and very bright sparks are obtained, the greatest length of which under favourable circumstances reach 25 to 26 centims. But at the ordinary temperature, without special heating, the length of the spark is 17 or 20 centims. ; and in a moist atmosphere the machine never remained active ; even sparks of a length of 10 centims. were produced.

It may be concluded from these facts that the ebonite machine equals the machine with glass disks ; besides, its power of retaining its electric charge is so high, that I often found it in electric condition when four weeks had elapsed ; and the action was entirely restored when the disk was set in rotation. It was even somewhat difficult to put the machine in a wholly neutral condition, if required for investigation ; a slight trace

of electricity that remained on the disks was sufficient to produce energetic sparks after a few revolutions of the disk. Neutralizing was in most cases effected when the horizontal conductors were brought into close contact and the disk was caused to rotate in the opposite direction to the former.

II. *The Double Electromachine with ebonite disks.*

The double electromachine consists of a combination of one fixed disk, on each side of which a somewhat smaller disk can rotate*. As the construction I have adopted is new, it may be useful to elucidate the description by a sketch in a horizontal projection (Pl. III. fig. 1).

Description of the apparatus.—On a wooden frame are fixed two pillars of ebonite, *a* and *b*. They bear two sets of horizontal conductors, *c*, *d*, and *c'*, *d'*; the latter are provided with electrodes, *e* and *f*, which can be put at different distances from another; *g* and *g'* are vertical metallic rods, which, when in contact with *e* and *f*, serve to convey the two electricities to other apparatus. *M* is a wooden column, which holds the horizontal axis *A*, a fixed steel rod on which an ebonite cylinder is caused to rotate by means of a band and wheel, *N*. This cylinder carries the two movable disks of ebonite, *p* and *q*, pressed together by small plates *n* and *n'* and the screw *z*; between them stands fixed on the board the disk *k*, which is retained in its position by screws *l*, *l'*, and *t* (fig. 2). A large aperture in its centre permits the axis *A* to rotate without communicating its motion to it. Finally, in *h h* and *h' h'* the two diametral or inclined conductors are shown, which can be moved to and fro if necessary. One (*h h*) is fixed on an ebonite ring, the other on the axis; *r r* and *r' r'* are two sets of paper armatures; fig. 2 shows their arrangement, near the apertures *o* and *o'*, on the fixed disk. All the parts are so adjusted that they may be easily removed; therefore the conductors *c* and *c'*, *d* and *d'* are separately screwed to the columns *a* and *b*; and when the screw *z* is loosened the disks can be taken away from the axis.

Exciting the machine.—This is effected in the same way as in the former apparatus, and as quickly. A peculiar method for bringing it into an active condition consists in causing the disks to rotate rapidly, rubbing one of them at the same time with the hand. It then becomes electric by friction; this is sufficient to produce a stream of sparks between the electrodes, and then the hand need no longer be applied.

* The first double electromachine was constructed by Dr. P. Kaiser, and described in *Les Mondes*, 1869. Another construction, also very powerful, was published by Prof. Poggendorff in his *Annalen*, 1871.

Results obtained with the machine.—In the apparatus here described, the dimensions are not very great; the fixed disk has a diameter of 35 centims., and the two rotating ones 30 centims. Between the electrodes (when the negative conductor terminates in a spheroid) a stream of sparks is produced, without using the condensers, at a distance of 6 centims.; in the opposite case, of 3 centims. With the condensers applied (which have a tinfoil armature of 90 centims. square) numerous sparks pass from one to the other electrode at a distance of 10 or 12 centims.; and in the most favourable circumstances (for instance, when a slowly burning coal fire is at hand) the electricities are discharged even at a distance of 17 centims.—that is, 2 centims. more than the radius of the rotating disk. This occurred especially when the loss of electricity by streaming out from the sharp points of the positive electrode was diminished by bringing it into contact with a small disk of ebonite, fixed on the board near *e* or *f*. I must add here, as an important property of this kind of electromachines, that the reversing of the electric condition of the paper armatures (which is very inconvenient in the single apparatus) never occurs in the double system. The only way to change the polarity of the electrodes is first to discharge the machine entirely, and then to excite again in the opposite manner. For the first-described machine the polarity could be reversed by moving the diametral conductor out of its position, and then bringing it back again; then the armatures always changed their electric condition.

III. *New Observations with Ebonite Machines.*

The properties of the diametral conductor.—The great difference between the electromachine with glass disks and those with ebonite ones, consists in the peculiarity that the latter absolutely require the diametral or inclined conductor, in order to become active and to remain so; by taking it away the machine is immediately rendered inactive.

On the contrary, it is sufficiently known that machines with glass disks may be used without the conductor, although they do not work as regularly, nor is the length of the spark as great, as when provided with it*. It has been stated by Prof. Poggendorff, that when no large paper armatures are used it is necessary, in machines with glass disks, to *take away* the diametral conductor; otherwise the machine cannot be excited. Now here it is quite the contrary: several times I

* Experimenters with the machines with glass disks will have had many occasions to observe that the length of the spark obtained without this conductor can be, with proper dimensions, 12 or 15 centims.

tried to get the machine active, by one of the methods mentioned above, without this conductor; but it remained no longer charged as when the exciting cause (the hand or the ebonite rod) was present: the apparatus would never continue its action till the diametral conductor was applied; and the action ceased on its removal.

This property may be used to show, in a very satisfactory manner, that the quantity of electricity that the double electromachine yields is really doubled. When one of the diametral conductors is removed, then the disk before which it was placed becomes as completely inactive as when it was taken out of the apparatus. Now, for instance in half a minute, by using two rotating disks, as many as 40 sparks (5 centims. in length) appeared between the electrodes; and after removing one of the diametral conductors almost exactly 20 sparks only were obtained. To get the machine active when the horizontal conductors were brought into contact, and without the inclined conductor, was impossible, because it always requires this piece; and experience confirms this; yet the machines with glass disks are easily charged in this way. I have not succeeded in accounting, in a quite satisfactory manner, for the remarkable behaviour of the diametral conductor in connexion with ebonite machines, which, as will now be clear, is quite the contrary of that with glass disks. The following remarks, however, may be made. When the ebonite machine is excited by placing an electric ebonite rod before one of the paper armatures, it is certain that, in consequence of the extremely bad conductivity of the material used, the induction through the disks on the points of the horizontal conductors will be very difficult to set in action, and that it is more advantageous, in order to excite the machines, to hold the electric body immediately before the points of the diametral conductor, which then charges the rotating disk with the two electricities. These, in turn, act by induction on the card points of the armatures; and in this way the apparatus is rapidly charged.

But the same might be supposed to take place without the diametral conductors, by means of the points of the horizontal conductors, when in contact with another; and a feeble action at least might be expected. This is really the case with the double electromachine when glass disks are used; it does not, however, become active so rapidly as in the ordinary way.

The theory which would account for this property of the diametral conductor in ebonite machines is yet a desideratum.

Action of the ebonite machine when rotating in an abnormal direction.—The ebonite machine of the first species possesses a

property which I have not found mentioned of the same apparatus with glass disks: it also develops electricity when the disk is rotating *in the direction of the card points*. I call this direction (which is just the contrary of the usual motion) *abnormal*.

When during some time energetic discharges have occurred, and the diametral conductor is removed from its position, so as to occupy a position as much on the left of the vertical as before on the right (it has then no armatures before its points), again a series of sparks appear between the horizontal conductors when the disks are rotating in the opposite direction to their former one; even sparks of a length of 5 centims. may then be obtained. The condensers are not necessary; and the same fact is observed with the double electromachines. I believe that the cause of it must be ascribed to the electricity which remains on the armatures, as these are perfectly insulated, and therefore the machine acts as an electrophorus machine. The action is not increasing; it is rather slowly diminishing. I observed in this case also the singular fact that when the diametral conductor occupies the described position, but the disks are rotating in the usual way (against the card points), the sparks cease to pass between the horizontal conductors; but often, in the same horizontal conductor, half the points become positively, and the other half negatively electrified, which is evident from their luminous appearance in the dark.

Action of the electricity on flames.—When trying to discharge the ebonite machine by approaching a lighted candle near the disks, I observed a remarkable influence exerted by the accumulated electricity on the shape of the flames. Near the paper armature charged with positive electricity (from whose points negative electricity is escaping) the flame is repelled strongly, and retreats from the disk; near the negative armature it is attracted, and ends in a sharp point. The same is seen with a gas-flame; and I even succeeded in substituting for the diametral conductor a so-called *flame conductor*, which consists of a wooden rod, whereon no points are fixed, but small glass tubes connected with the gas-pipes of the laboratory. A set of flames is in this way produced before the rotating disk, which, by their forms, denote its positive and negative conditions.

The explanation of this experiment may be founded on the fact, not very generally known (mentioned by Riess), that when a substance is burnt electricity is generated; and in the case of carbon being present, the ascending carbonic acid is positively electrified.

We may conclude from the foregoing statements:—

1. The ebonite electromachines, when constructed with ebonite of good quality, are at least equal to the best machines with glass disks in regard to their action; but they surpass them by their relatively small expense, nonfrangibility, and permanent electric condition.

2. The double electromachine constructed by me is a very powerful arrangement for producing electricity in great quantity, as well as of great tension.

3. The substitution of ebonite for glass disks not only deserves attention because the usefulness is enhanced, but also because new data are acquired, which may be of great interest for the complete theory of electromachines.

The Hague, March 1876.

XLIII. *On Salt Solutions and Attached Water.*

By FREDERICK GUTHRIE.

[Continued from p. 60.]

IV.

Separation of Ice, or of a Hydrate, or of the Anhydrous Salt, from solutions of Salts below 0° C.

§ 123. **V**ERY many determinations have been made of the solubility of various salts in water at temperatures ranging from 0° C. to the temperature of ebullition of the saturated salt solution. To Gay-Lussac, to Kremers, to Regnault, and others are due several series of exact determinations in this direction of research. But the examination of the phenomena of solidification which take place in salts of certain strengths at temperatures below 0° C. has suffered comparative, almost complete, neglect. And this appears to have arisen partly from the circumstance that it has been asserted by some philosophers, and denied by others entitled to equal respect, that when a solution of a salt below 0° C. begins to solidify by loss of heat, the solid formed is pure ice. It has been my privilege to reconcile the opposing views by the discovery of the fact which I trust now to have fully established, that when a salt solution which is already any fraction of a degree below 0° C. is cooled, one of three things must happen; and which of them happens is determined with the same salt by the strength of the solution.

1. In all solutions weaker than the cryohydrate, ice is formed, at temperatures which are lower according as the solution is richer in salt.

2. In solutions of a certain strength (namely that of the cryohydrate), combination of the salt and water take place in definite ratio and at a constant temperature. The solution is therefore a melted cryohydrate, and solidifies as a whole.

3. When solutions stronger than the cryohydrate, below 0° C., are cooled, either the anhydrous salt or some hydrate richer in salt than the cryohydrate separates.

It follows that the cryohydrate may, and indeed must inevitably be reached both by cooling a solution weaker than the cryohydrate (ice separates, the solution strengthens, the temperature sinks), and by cooling a solution stronger than the cryohydrate (anhydrous salt or a hydrate richer than the cryohydrate separates, the solution weakens, the temperature sinks). Assuming as a fact that which will be abundantly proved, namely that every salt which is soluble above 0° C. is also soluble below 0° C. (we shall see that there is even no quantitative discontinuity at 0° C. in the solubility), it follows immediately that a solution of any degree of strength whatever gives rise on cooling, sooner or later, to a liquid which remains unsolidified until the proper ratio and temperature is reached, and then solidifies in that constant ratio and at that constant temperature. From a solution of the strength of the cryohydrate nothing separates until the proper temperature is reached. And then, however rapid the absorption of heat may be, the temperature never sinks lower until solidification is complete.

§ 124. I propose in the present communication (1) to trace the history of solutions weaker than the cryohydrates as they yield ice on cooling, and (2) to examine the separation of anhydrous salts or hydrates richer than the cryohydrates when solutions richer than the cryohydrate are cooled. With regard to the first of these branches of inquiry, the only experiments in this direction which I have described are those relating to chloride of sodium in § 10, and those concerning spirits of wine in § 96.

A solution of a salt below 0° C. which is stronger than the cryohydrate, may be viewed as a solution in the cryohydrate, either of a hydrate richer than the cryohydrate or of the anhydrous salt. A solution of a salt below 0° C. which is weaker than the cryohydrate may be properly regarded as a solution of ice in the cryohydrate. A solution of chloride of sodium stronger than its cryohydrate may be also regarded as a solution of the subcryohydrate, $\text{Na Cl } 2 \text{ H}_2 \text{ O } (?)$, in the cryohydrate (§ 15).

Just as a given weight of water dissolves at temperatures above 0° C., with rare exceptions, more of a salt the higher the temperature, so a given weight of the cryohydrate dissolves more ice at higher temperatures, below 0° C., than at lower ones. The weaker a given weight of a salt solution below 0° C. is, the smaller is the weight of the cryohydrate in it, and accordingly the less is the weight of ice which the given weight

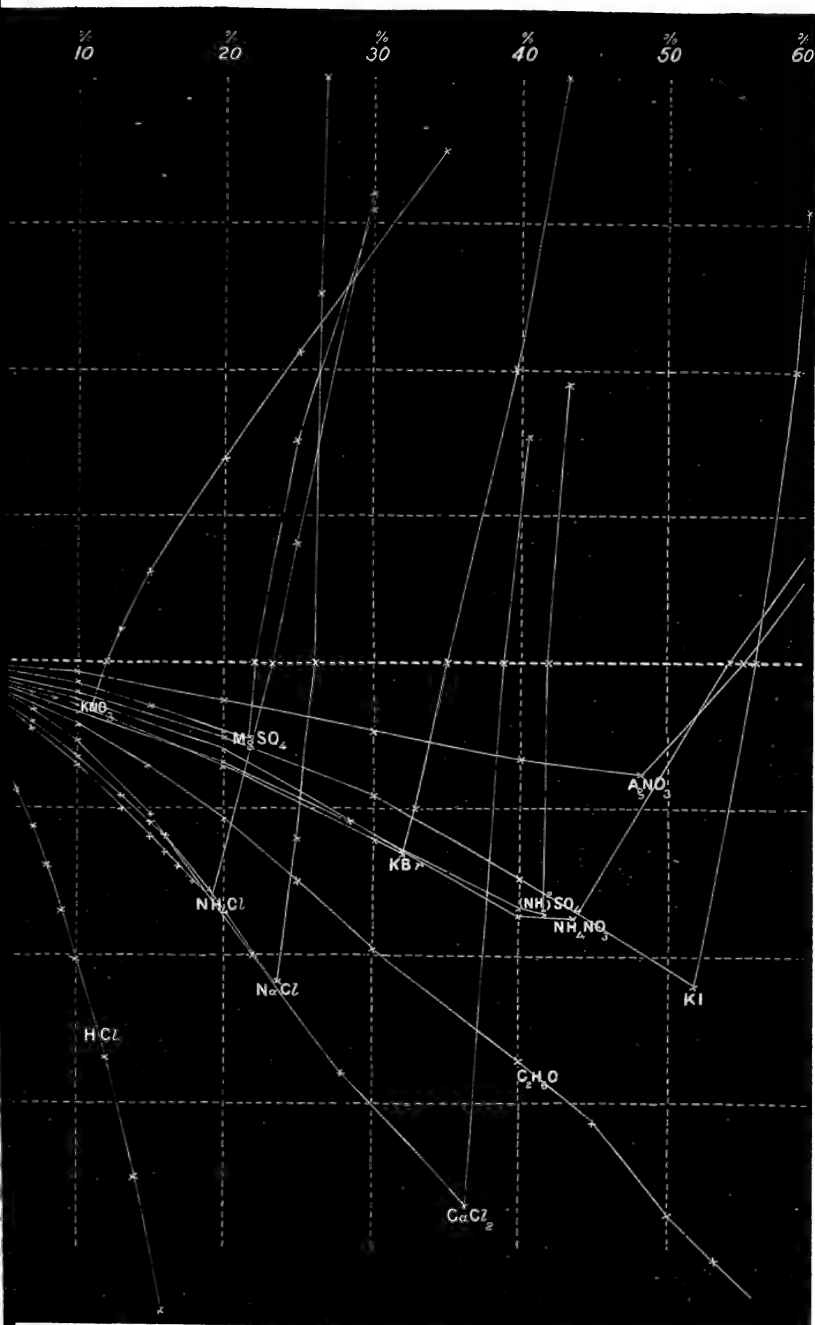
of solution is capable of dissolving. Experimentally, this appears in the results of all my experiments to be described below, that the stronger a solution of any given salt is (being weaker than the cryohydrate), the lower is the temperature below 0° C. to which it must be brought in order to yield ice; and the stronger a solution of any given salt is (being stronger than the cryohydrate), the less need its temperature be depressed in order to yield the anhydrous salt, or a hydrate richer in salt than the cryohydrate. And, as before stated, the latter kind of separation is quite continuous with the separation of such bodies at temperatures above 0° , such as is exhibited in the ordinary Tables of solubilities referred to in § 123.

§ 125. Almost all the numerical results are represented graphically in fig. 1. The abscissæ are in all cases the percentages of the anhydrous salt in the water. The ordinates, — or +, are the temperatures Centigrade below or above 0° C. at which solid matter begins to separate on cooling the solution. The point of reflexure (that is, the lowest point of each tracing) shows the temperature at which the particular cryohydrate is formed, and also the percentage of anhydrous salt in the cryohydrate. The left-hand branches of the tracings exhibit the temperature at which ice separates from solutions of the respective salts of the corresponding percentage composition. The right-hand branch of each tracing shows the temperatures at which the anhydrous salt or a hydrate richer than the cryohydrate is separated. The parts of these branches above the zero-line show the ordinary solubility of the same salts above 0° . The figure is reduced from my drawing, in which the percentages and tenths were centimetres and millimetres; and the degrees C. and their tenths were also centimetres and millimetres. The actual observations, being dotted off, were joined by straight lines.

§ 126. *General remarks on the experiments.*—Whenever possible, the solutions of the salts examined were made by dissolving weighed quantities of the anhydrous pure salt in quantities of water measured from a Mohr's burette. The grammes and centimetres were those of the percentages; that is, to form a 20-per-cent. solution, 20 grms. of the salt and 80 cubic centims. of distilled water were employed. Now and then a control mixture formed by weighing only was examined; but it appeared that the sources of error introduced by measuring instead of weighing the water were vanishingly small compared with the unavoidable errors of observation, notably those mentioned in the next paragraph.

Supersaturation.—With every salt solution from which we wish to get solidification by means of cooling, we have to guard

Fig. 1.



against supersaturation. The notion that in order to start the crystallization of a supersaturated solution crystals must be present in the air identical in chemical kind with those ready to be formed in the solution, seems to be disproved by the fact that supersaturation in regard to ice is of the very commonest occurrence. In many cases a salt solution which should yield ice at a temperature $-t^{\circ}$ refuses to do so until $-(t^{\circ} + 5^{\circ} \text{ or } 6^{\circ})$ is reached; then ice-crystals are formed through the mass and the temperature rises. Although we may indeed imagine ice-crystals to be floating in the air about the surface of the cryogen, we can scarcely conceive these microscopic crystals to travel unmelted through the surrounding air, which may be at 10° or 15° C., so as to reach the liquid. And if it be argued that the origin of the ice-crystals may be at or near the surface of the salt solution in the experimental tube, I reply that crystallization from a supersaturated solution occurs both when it is covered with a layer of oil and when only the lower part of such a solution is acted on by the cryogen.

Method of observation.—Since the separation of ice strengthens the solution, it is necessary to determine the temperature of the very beginning of the ice-formation. The salt solution is cooled in a test-tube until some ice is formed; this is very nearly completely remelted under constant stirring with the thermometer, and then the tube is plunged momentarily into the cryogen. The minute spicula of ice so formed are again nearly remelted. The mean of four or five readings of the thermometer when the minute quantity of ice begins to increase is taken as the true temperature of ice-formation. In order to save time and start the crystallization of ice in a solution which one has reason to suppose is supersaturated with ice, the thermometer-bulb may be plunged into the cryogen and rapidly wiped dry, or into the mercury-cup to be described immediately. It soon becomes clothed with a film of ice-crystals from the moisture of the air. These are indeed almost invisible, and far too small in quantity to alter the strength of the salt solution when brought into contact with it, but amply sufficient to determine the separation of ice if the solution be supersaturated in regard to that body.

When the tube containing the salt solution is plunged into the cryogen till crystallization begins and then removed, portions of the cryogen adhering to the tube may carry the ice-formation too far, and by soiling obscure the tube. It is therefore found convenient to have standing in the freezing-mixture a short wide-mouthed bottle of mercury, and for the final observations to plunge the tube into this and so avoid soiling.

The solutions stronger than the cryohydrate were treated in a similar manner.

For the solubilities of the various salts at 0° I kept the solu-

tions, previously saturated at 12° C., for three hours at 0° , and determined the strengths by evaporation &c., in some cases by determination of one constituent by the usual chemical means. I find these latter determinations agree very closely with those of Kremers throughout. Indeed, as will be seen, we seldom differ more than 0.3 per cent. For the determination of the temperatures above 0° C. at which salts and their hydrates separate, I have found, contrary to my anticipations, the more exact method to be in most cases to warm a given weight of the salt with its proper percentage of water in a stoppered bottle till solution was effected, and then by repeated observation find the temperature of incipient crystallization, rather than attempt to keep the water and salt for a length of time at a constant temperature and then determine the strength. The latter method, however, is of course especially available for 0° and 100° C.

§ 127. *Separation of Ice, of Subcryohydrate and of NaCl from solutions of that salt.*—The following Table (XVI.) shows the temperature below 0° C. at which ice begins to be separated from solutions of common salt of different strengths. In selecting these strengths I have chosen several close to the critical points, namely to that of distilled water, to that of the cryohydrate, and to that of saturation. And in the case of other salts I have, as far as possible, examined solutions of the same percentage-weight strengths. The ascent (see fig. 1. NaCl) is so steep from the cryohydrate to 0° C. that I have only been able to introduce one observation. As my determination for 0° C. was 26.27 (§ 11), and Poggiale's was 26.28 , I have no hesitation in adopting Poggiale's for $+25^{\circ}$ and $+40^{\circ}$.

TABLE XVI.

NaCl per cent. by weight.	H ₂ O per cent. by weight.	Temperature Centigrade at which a solid is formed.	Nature of solid.
1	99	— 0.3	Ice.
2	98	— 0.9	"
3	97	— 1.5	"
4	96	— 2.2	"
7	93	— 4.2	"
10	90	— 6.6	"
13	87	— 9.1	"
15	85	— 11.0	"
16	84	— 11.9	"
19	81	— 15.5	"
20	80	— 17.0	"
22	78	— 20.0	"
23.6	76.4	— 22.0	Cryohydrate.
25	75	— 12.0	{ Subcryohydrate.
26.27	73.73	0.0	{ NaCl.
26.5	73.5	+ 25.0	" (Poggiale).
26.8	73.2	+ 40.0	" (Poggiale).

This Table is to be regarded as replacing Table VI. in § 10 as far as the two are comparable. I bracket the subcryohydrate and the NaCl together, because the range of ratio is so small for a great temperature-difference that I cannot at present undertake to say within two or three degrees the temperature-range peculiar to each body.

§ 128. *Separation of Ice or Anhydrous Chloride of Ammonium from solutions of that salt.*—The following Table (XVII.) shows the temperature of solidification of solutions of NH_4Cl of various strengths.

TABLE XVII.

NH_4Cl per cent. by weight.	H_2O per cent. by weight.	Temperature Centigrade at which solidifi- cation begins.	Nature of solid.
1	99	— 0.4	Ice.
3	97	— 1.6	"
5	95	— 3.1	"
7	93	— 4.6	"
10	90	— 7.1	"
13	87	— 9.9	"
15	85	— 12.0	"
16	84	— 13.0	"
17	83	— 14.0	"
18	82	— 15.0	"
19	81	— 15.8	"
19.27	80.73	— 16.0	Cryohydrate.
20	80	— 15	NH_4Cl .
22	78	— 5	"
23.2	76.8	0	"
25	75	+ 8	"
30	70	+ 32	"

It will be seen that I make now the solidifying-point of the cryohydrate —16 instead of —15. This agrees with its temperature as a cryogen. The tracing of NH_4Cl in fig. 1 is seen to pass through the 0° line without deflection. Like the NaCl line it has a point of contrariflexure, but not so near the cryohydrate as with the latter salt. The determination at 0°C . was derived from the analysis, 12.1852 of the liquid gave 2.7721 of NH_4Cl .

§ 129. *Separation of Ice or Anhydrous Nitrate of Potassium from solutions of that salt.*—Nitrate of potassium being far less soluble below 0°C . than the two preceding salts, and its cryohydrate not differing much from the saturated solution at 0° , I have not attempted to get any temperature-values between the two; but I have carefully followed the form of the tracing up to the cryohydrate, and also above 0° .

TABLE XVIII.

KNO_3 per cent. by weight.	H_2O per cent. by weight.	Temperature Centigrade at which solidifi- cation begins.	Nature of solid.
1	99	-0.1	Ice.
2	98	-0.3	"
3	97	-0.7	"
4	96	-1.1	"
5	95	-1.5	"
7	93	-2.2	"
8.5	91.5	-2.6	"
10	90	-2.9	"
11.2	88.8	-3.0	Cryohydrate*.
12	88	0.0	KNO_3 .
13	87	+2.0	"
15	85	+6.0	"
20	80	+14.0	"
25	75	+21.0	"
35	65	+35.0	"
40	60	+41.0	"

The composition at 0°C . was derived from the analysis by evaporation of 11.8050 grms., which gave 1.4166 of KNO_3 , or 12 per cent. (Gay-Lussac found 11.68).

§ 130. *Separation of Ice and Hydrate of CaCl_2 from solutions of that salt.*—Although troublesome to manipulate, I preferred to make the solutions by weighing the anhydrous chloride.

TABLE XIX.

CaCl_2 per cent. by weight.	H_2O per cent. by weight.	Temperature at which soli- dification begins.	Nature of solid.
1	99	0.2	Ice.
2	98	0.5	"
3	97	1.1	"
4	96	1.6	"
5	95	2.1	"
7	93	3.3	"
10	90	5.5	"
15	85	10.5	"
20	80	17.5	"
28	72	27.5	"
36.45	73.5	37	Cryohydrate.
39	61	0	Unknown hydrate.
45 (Gerlach)	55	15.5	$\text{CaCl}_2 + 6\text{H}_2\text{O}$.
			"

* I have adopted the temperature -3° , after a long series of experiments, instead of $-2^\circ.6$, which is given in Table X.

The determination at 0° C. was made by estimating the chlorine as chloride of silver. There is such little difference between the composition of the cryohydrate and the hydrate separated at 0° , that I have been unable to estimate the composition at intermediate temperatures. There is no evidence of the formation of any other hydrate than the one usually regarded as containing six molecules of water. On account of the difficulty of exactly measuring such low temperatures as are here concerned at the maximum, I have less confidence in the exactness of the form of the tracing of this salt than of any other.

§ 131. *Separation of Ice from a solution of Hydrochloric Acid.*—Although I have made a separate study of the hydrogen salts of several of the acids, I may here adduce one instance, namely that of hydrochloric acid, in order especially to compare the forms of the temperature-curve of a body which is, as far as we know, without a cryohydrate*, with the curve-forms of metallic salts, all of which have cryohydrates within the range of artificial cold.

By a silver determination I found that 3.2726 grms. of a pure acid contained 1 gram. of HCl and 2.7734 grms. of water. From this acids of unit percentage weights were formed.

TABLE XX.

HCl per cent. by weight.	H ₂ O per cent. by weight.	Greatest cold as cryogen.	Temperature at which soli- dification begins.	Nature of solid.
1	99	°	— 0.7	Ice.
2	98	— 2.0	"
3	97	— 2.0	— 3.6	"
4	96	— 5.3	"
5	95	— 2.6	— 7.0	"
6	94	— 9.0	"
7	93	— 4.0	— 11.5	"
8	92	— 14.0	"
9	91	— 4.6	— 17.0	"
10	90	— 4.9	— 20.5	"
12	88	— 6.2	— 27.0	"
14	86	— 7.6	— 35.0	"
16	84	— 8.0	— 45.0	"

The tracing of these values is shown on fig. 1, and may be compared with that of spirit of wine derived from the numbers given in § 96, Table XI. In neither tracing is there any decided indication of contrariflexure, although the spirit-

* A cryohydrate of HCl has recently been described by a French chemist.

of-wine tracing passes through its cryohydrate. In the centre column of Table XX. is given the greatest cold when used as cryogens that these acids can produce. The acid was in all cases at 12° C. The great specific heat of the water in which the HCl is dissolved prevents, of course, the attainment of the degree of cold equal to that necessary to solidify a cryohydrate, as is the case with those true salts which do not heat on hydration.

§ 132. *Separation of Ice or Anhydrous Bromide of Potassium from solutions of that salt.*

TABLE XXI.

KBr per cent. by weight.	H ₂ O per cent. by weight.	Temperature Centigrade at which solidifi- cation begins.	Nature of solid formed.
10	90	- $\overset{\circ}{3}$	Ice.
20	80	- 7.1	"
30	70	- 12.0	"
32.15	67.9	- 13.0	Cryohydrate.
33	67	- 9.8	KBr.
34	66	- 5.0	"
35.03	65	0	"
(Kr.) 39.7	60.3	+ 20	"
(Kr.) 43.2	56.8	+ 40	"

The solubility at 0° was found from an experiment in which 11.2939 grms. of solution gave 3.9566 grms. of KBr. As Kremers found 34.5, I have adapted his determinations at $+20^{\circ}$ and $+40^{\circ}$ by adding 0.5 to the numbers he gives.

§ 133. *Separation of Ice and Anhydrous Iodide of Potassium from solutions of that salt.*

TABLE XXII.

KI per cent. by weight.	H ₂ O per cent. by weight.	Temperature Centigrade at which solidifi- cation begins.	Nature of solid formed.
10	90	- $\overset{\circ}{2.2}$	Ice.
20	80	- 5.1	"
30	70	- 9.0	"
40	60	- 14.4	"
52.07	47.93	- 22	Cryohydrate.
55.93	44.07	0	KI.
(Kr.) 58.9	41.1	+ 20	"
(Kr.) 61.4	38.6	+ 40	"

Having found that 10.3681 grms. of the solution saturated

at 0° C. contained 5.7990 of KI, or 55.93 per cent., and since Kremers found 55.86, I have adapted his determination for the 20° and 40° temperatures by adding 0.1 per cent. to his results. The ascent of the tracing (fig. 1) between the cryohydrate and 0° is too abrupt for the insertion of any intermediate value which shall be trustworthy.

§ 134. *Separation of Ice and Anhydrous Sulphate of Ammonium from solutions of that salt.*—The salt was dried at about 100° C., finely powdered, kept at 100° C. for ten hours, and kept for three days and nights in an exhausted receiver over sulphuric acid.

TABLE XXIII.

$(\text{NH}_4)_2\text{SO}_4$ per cent. by weight.	H_2O per cent. by weight.	Temperature Centigrade at which solidifi- cation begins.	Nature of solid formed.
10	90	— 2.6	Ice.
20	80	— 6.0	"
28.6	71.4	— 10.8	"
40	60	— 16.0	"
41.7	58.3	— 17.0	Cryohydrate.
41.9	58.1	0.0	$(\text{NH}_4)_2\text{SO}_4$.
(H.S.) 43.2	56.8	+ 19.0	"

I have for the temperature 19° adopted H. Schiff's determination. Of all the salts which I have examined, the sulphate of ammonium shows in its tracing the greatest precipitousness in its ascent from the cryohydrate to the 0° C. ratio. It is in this region undoubtedly very nearly a straight line. The solubility at 0° C. was found from the data, 13.2512 of the solution gave 5.5522 of the salt obtained anhydrous by the means mentioned above.

§ 135. *Separation of Ice or Nitrate of Ammonium from solutions of that salt.*—The nitrate of ammonium used was dried at 100° C., finely crushed, and heated to incipient fusion; it ceased to lose weight but showed no trace of the nitrite. In the determination of the solubility at 0° C. it was treated in the same manner.

TABLE XXIV.

NH_4NO_3 per cent. by weight.	H_2O per cent. by weight.	Temperature Centigrade at which solidifi- cation begins.	Nature of solid formed.
10	90	- 3.5	Ice.
20	80	- 7.0	"
30	70	- 11.5	"
40	60	- 17.0	"
43.7	56.3	- 17.2	Cryohydrate.
47	53	- 12.0	NH_4NO_3 .
51	49	- 5.7	"
54.1	45.9	0.0	"
(Kar.) 66.5	43.5	+ 18.1	"

The solubility at $18^\circ.1$ is the observation of Karsten. The shape of the tracing of this salt bears a remarkable resemblance to that of nitrate of potassium (see fig. 1). The gradual slope of its right-hand branch enabled me to insert two determinations there. Also noteworthy is the comparison between the tracing of this salt and that of the sulphate of the same base. Running together with near coincidence down to their cryohydrates, which are nearly identical in temperature and not far apart in percentage of salt, the one rises more abruptly than any other curve; the other is, with one exception, the most gradual.

§ 136. *Separation of Ice and Hydrated Sulphate of Magnesium from the solutions of that salt.*—The analysis of crystallized sulphate of magnesium in regard to its water by direct estimation is rather unsafe, on account of the high temperature required to drive off the last traces of water, and the decomposition of the salt at a somewhat higher temperature. I therefore determined the sulphuric acid in a sample of the sulphate I was about to use, and found that it was nearly the so-called hepthhydrate. The determination of the solubility at 0°C. was also derived from a sulphuric-acid determination. Taking every 1.96 grm. of the crystallized salt as containing 1 grm. of MgSO_4 , the following solutions were examined:—

TABLE XXV.

MgSO ₄ per cent. by weight.	H ₂ O per cent. by weight.	Temperature T at which solidification begins.	Nature of solid formed.
5	95	— 0·6	Ice.
10	90	— 1·5	"
15	85	— 3·0	"
20	80	— 4·8	"
21·86	78·14	— 5·0	Cryohydrate.
21·9	78·1	0·0	MgSO ₄ + 7H ₂ O.
25	75	+ 15·0	"
30	70	+ 31·0	"

As my determination of the solubility at 0° C. (21·9) differs considerably from some of those given by others (Gay-Lussac 20·5, Graham-Otto 20·5, Pfaff 21·9), I determined for myself the solubilities above 0°.

§ 137. *Separation of Ice or Cryohydrate or Anhydrous Nitrate of Silver from a solution of that salt.*—I have not yet described the temperature and composition of the cryohydrate of this salt. A solution saturated at 0° C. and further cooled gives rise to an abundant crop of iridescent scales, which on examination prove to be the anhydrous nitrate. This continues till —6°·5, when the opaque cryohydrate is formed. The solution, however, is particularly prone to double supersaturation. A solution may sometimes be cooled to —8°; and during this, while the crystals of the nitrate fall, ice-crystals are formed which float. By and by the true cryohydrate is formed on the side of the glass; and then both separate crystallizations cease. This double supersaturation is entirely prevented by introducing a fragment of the previously formed cryohydrate into the mother-liquor which is being further cooled. The temperature remains then constant at —6°·6 to the last drop. The final crop being remelted gave, on analysis, the following results:—14·3538 grms. gave 6·9442 grms. of AgNO₃, or 48·38 per cent. of AgNO₃ in the cryohydrate. The cryohydrate has accordingly the composition AgNO₃ + 10·09 H₂O. The temperature —6°·5 is obtained on employing nitrate of silver as a cryogen. There are few more beautiful colourless bodies than the cryohydrate of silver. Opaque, lustrous, and semimetallic, it is somewhat heavier than the liquid in which it is formed.

Of a solution saturated at 0° I found that 6·3581 grms. gave 3·5068 of the anhydrous nitrate, or 55·01 per cent. Kremers found 54·9; and accordingly I have availed myself of his deter-

minations for the solubilities above 0° C. by adding 0.1 to his percentages.

TABLE XXVI.

AgNO ₃ per cent. by weight.	H ₂ O per cent. by weight.	Temperature at which soli- dification begins.	Nature of solid formed.
10	90	— 0.8	Ice.
20	80	— 2.7	"
30	70	— 4.7	"
40	60	— 6.0	"
48.3	51.7	— 6.5	Cryohydrate.
50	50	— 5.5	AgNO ₃ .
53	47	— 2.2	"
55	45	0.0	"
(Kr.) 69.4	30.4	+ 19.5	"

§ 138. *Thoughts suggested by fig. 1.*—As the whole field of inquiry contained below the zero-line of fig. 1 has, I believe, remained hitherto unexplored, I am tempted to submit a few remarks upon it. First, of course, the tracings should be continuous curves between 0° and the cryohydrates, and again between the latter and the boiling-point of the saturated solutions, and doubtless, under pressure, beyond. Further, it seems that the curves are all varieties of the same kind. The figure 2 shows what we may liken to the backbone of the curve, with a typical curve about it. There is no absolute measure of what may be called the eccentricity of the curve, because there is no common measure between percentage composition and temperature; both are measured in arbitrary units. But this does not affect the type or order of the curve's equation. On the left-hand branches there appears to be a point of contrariflexure. The only apparent exceptions to this are KI, alcohol, and HCl. Concerning the two latter, doubtless we only see a fragment of their curves before their final cryohydrates. If there be such a point with KI it must lie between 40 per cent. and the cryohydrate, which is well possible. There seems to be a point of contrariflexure also on the right-hand branch, though sometimes the curvature is so small that this point is practically obliterated. The average slope of descent to the cryohydrate is invariably less than

Fig. 2.

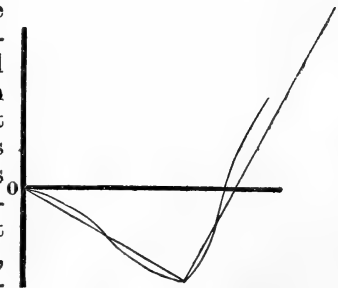


Fig. 2. The curve shows a minimum point, indicating a point of contrariflexure. The curve is labeled 'Fig. 2.'

the slope of ascent thence to zero. Indeed every tangent to the left-hand branch of each curve makes a more acute angle with the zero axis than any tangent to the right-hand branch of the same curve. If this be true throughout, the unknown right-hand branch of the HCl curve must be very nearly at right angles to the axis of 0° C. There is no evidence to show that the right-hand branch is continuous with the left-hand. In the case of the halogen salts this almost appears to be distinctly shown not to be the case. If the branches be continuous, it appears that the colder the cryohydrate the less the curvature there. If the branches be discontinuous, the colder the cryohydrate the more acute is the angle between the tangents at the intersection of the curves. This rule holds good in all cases; but between NaCl and KI there is considerable difference of sharpness at the cryohydrate, attending identity of temperature. It is not the case that the deeper the valley the more precipitous the ascent, travelling from left to right; nor is it true that the deepest valley has the widest mouth. That is, salts the most soluble at 0° do not have the coldest cryohydrates, and therefore do not form the most powerful cryogens.

In the shape of the bottoms of the valleys (that is, the curvature about the cryohydrates) the curves of the oxygen salts show a likeness to one another as distinguished from the halogen salts. Considering the former by themselves, we might be inclined to view their two branches as being in all cases continuous. To settle the question decisively whether these curves are continuous, hundredths of degrees would have to be read with precision, and the initial formation of ice noted within that range. If we judge by the aspect of the halogen curves and the general continuity in properties between the halogen and the oxygen salts, I am inclined to consider the two branches discontinuous in all cases.

The tracings assume, of course, somewhat different positions if they are referred to molecular ratio instead of to absolute-weight ratio. Without determining each tracing for all its intermediate values, we may join by straight lines the points representing the molecular solubility at 0° with the molecular value of the cryohydrate. While each of these lines is, of course, parallel to the corresponding line derived from fig. 1, being got by determining its extremities by division of the percentage by the molecular weight, such division necessarily brings those lines relatively further towards the left whose salts have heavy molecules. The extreme points of the lines are got by dividing the percentage values of fig. 1 by the molecular weights and multiplying by 100. I know no property of the salts here under examination which would have

enabled us to predict the position and inclination of these lines. Neither are the water-worths of the salts as cryohydrates nor their solubilities in water at 0° C. in any obvious accord with their known properties.

[To be continued.]

XLIV. *On the Proof of the Second Law of Thermodynamics.*

By R. C. NICHOLS.*

IN the last January Number of the Philosophical Magazine appeared two papers intended to demonstrate what is called the second law of thermodynamics, or the principle of Carnot and Clausius, by showing that the differential of the quantity of heat communicated to a gas, and thereby causing work to be done, divided by the temperature, is an exact differential. The paper of Mr. Burbury proceeds upon the basis of the theory of Boltzmann, and demonstrates that this conclusion can be deduced from Boltzmann's results by a simpler process than that made use of by M. Boltzmann himself. That of M. Szily proposes to establish the same conclusion upon no other assumption than the fundamental principle of the conservation of energy.

To M. Szily's argument I have to offer the following objection. He says (p. 29):—"In a definite passage out of $(\xi_0\eta_0)$ into $(\xi_1\eta_1)$ the quantity of energy is a certain function of the time. The passage, however, from the same initial to the same final state, by the same path, can, with respect to time, be executed quite arbitrarily; consequently $\delta\epsilon$ is a different function of the time, according to the velocity of the importation of energy. By suitably selecting this velocity, any indefinitely small value whatever may be assigned to $\int_0^t \delta\epsilon dt$. Let

$$\int_0^t \delta\epsilon dt = t \delta Q."$$

Upon this I have to remark that $\delta\epsilon$ is in this sense a different function of the time according to the velocity of the importation of energy—that it is a function, not of the absolute time, but of the proportion of the whole time which has elapsed; so that $\int_0^t \delta\epsilon dt$ or $\overline{\delta\epsilon}t$ is a quantity which varies as t . On the other hand, δQ being, as already stated, absolutely independent of t , $t\delta Q$ likewise varies as t . Unless therefore $\delta Q = \overline{\delta\epsilon}$, for which

* Communicated by the Author.

supposition no ground whatever has been alleged, the assumption that $\int_0^t \delta \epsilon dt = t \delta Q$ is purely arbitrary, and moreover is untrue for any value of t unless it is also true for every value of t .

The general condition under which the proposition is true which M. Szily has sought to establish, and which has been proved by Mr. Burbury for the case in which the potential energy of the system has the special value deduced from the theory of M. Boltzmann, may be shown in the following manner.

Let it be observed that the addition of energy dQ is expended (1) upon alteration of kinetic energy, which call dT , (2) upon alteration of potential energy or dU , and (3) upon work done or dX ; and dQ must be equal to the sum of these quantities, or

$$dQ = dT + dU + dX. \quad . \quad . \quad . \quad . \quad (1)$$

Now if v be the volume of the gas and p its elastic force, or the force resisting expansion per unit of surface, dv the change of volume,

$$dX = p dv. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

And by the equation of Clausius,

$$pv = \frac{2}{3} (T + \Sigma \Sigma (\frac{1}{2} Rr)),$$

$$\therefore p dv = \frac{2}{3} T \frac{dv}{v} + \frac{1}{3} \Sigma \Sigma (Rr) \frac{dv}{v}$$

$$= p_1 dv + p_2 dv;$$

where $p_1 = \frac{2}{3} \frac{T}{v}$ is the element of the elastic force resulting from

the kinetic energy of the particles, and $p_2 = \frac{1}{3} \Sigma \Sigma \left(\frac{Rr}{v} \right)$ is the element resulting from the internal forces. Now supposing the change of volume from v to $v + dv$ were to take place in such a manner that the distances of the particles should, on the average, preserve the same relative amount, every dr resulting from the change of volume would equal $\frac{1}{3} r \frac{dv}{v}$,

or $p_2 dv$ would be equal to $\Sigma \Sigma \left(R \frac{dr}{dv} \right) dv$, or $\frac{dU}{dv} dv$. It is there-

fore equal to $\frac{dU'}{dv} dv$, where U' denotes a function of (v, T) , which varies as the potential of the system would do upon this supposition. Therefore

$$p dv = \frac{2}{3} T \frac{dv}{v} - \frac{dU'}{dv} dv. \quad (3)$$

And

$$dU = \frac{dU}{dv} dv + \frac{dU}{dT} dT,$$

$$\therefore dQ = dT + \frac{2}{3} T \frac{dv}{v} + \frac{dU}{dv} dv - \frac{dU'}{dv} dv + \frac{dU}{dT} dT;$$

and

$$\frac{dQ}{T} = d \log T + \frac{2}{3} d \log v + \frac{1}{T} \left(\frac{dU}{dv} - \frac{dU'}{dv} \right) dv + \frac{dU}{dT} d \log T. \quad (4)$$

In order that this may represent a complete differential, $\frac{1}{T} \left(\frac{dU}{dv} - \frac{dU'}{dv} \right) dv + \frac{dU}{dT} d \log T$ must be a complete differential of some function of v and T . Let u be this function, then

$$\frac{1}{T} \left(\frac{dU}{dv} - \frac{dU'}{dv} \right) dv + \frac{1}{T} \frac{dU}{dT} dT = \frac{du}{dv} dv + \frac{du}{dT} dT;$$

$$\therefore \frac{du}{dv} = \frac{1}{T} \left(\frac{dU}{dv} - \frac{dU'}{dv} \right),$$

and

$$\frac{du}{dT} = \frac{1}{T} \frac{dU}{dT};$$

and as

$$\frac{d}{dT} \frac{du}{dv} = \frac{d}{dv} \frac{du}{dT},$$

$$\frac{1}{T} \left(\frac{d}{dT} \frac{dU}{dv} - \frac{d}{dT} \frac{dU'}{dv} \right) - \frac{1}{T^2} \left(\frac{dU}{dv} - \frac{dU'}{dv} \right) = \frac{1}{T} \frac{d}{dv} \frac{dU}{dT};$$

$$\therefore \frac{1}{T} \left(\frac{dU}{dv} - \frac{dU'}{dv} \right) = - \frac{d}{dT} \frac{dU'}{dv}, \quad (5)^*$$

which is the condition necessary to make $\frac{dQ}{T}$ a complete differential.

Then

$$\frac{dQ}{T} = d \log T + \frac{2}{3} d \log v - \frac{d}{dT} \frac{dU'}{dv} dv + \frac{dU}{dT} d \log T \quad (6)$$

$$= d(\log T + \frac{2}{3} \log v + u). \quad (7)$$

If, however, such a relation should exist between the volume

* The original determination of this necessary condition of any system to which the second law may apply is due to Mr. S. H. Burbury. In the notation of M. Boltzmann $\frac{dU}{dv}$ is represented by $\frac{\bar{d}U}{\bar{d}v}$, and $\frac{dU'}{dv}$ by $\frac{\bar{d}U'}{\bar{d}v}$.

and temperature that v may be expressed as $f(T)$, $\frac{dQ}{T}$ must in any case be a complete differential. This latter relation is assumed by M. Szily when he puts $\eta=f(\xi)$ or $f'(\xi)$, taking for granted that the path of the state must be capable of expression by some such equation. In a paper "On the Moving Force of Heat," printed in the Philosophical Magazine in 1851 ([IV.] vol. ii. p. 1), M. Clausius pointed out that under such circumstances dQ also is a complete differential.

The hypothesis of Boltzmann gives $\frac{dU}{dv} - \frac{dU'}{dv} = -T \frac{d}{dT} \frac{dU'}{dv}$, but is not necessarily the only hypothesis which will satisfy this condition and make $\frac{dQ}{T}$ a complete differential independently of any constant relation between v and T .

The equations above obtained require some modification to meet the case of a gas supposed to consist of molecules the elements of which possess internal energy.

There are two ways of applying the formula of Clausius (which in any case holds good) to such a gas. (1) We may consider T to represent the kinetic energy of the molecules only, and $\Sigma\Sigma(\frac{1}{2}Rr)$ the virials of the intermolecular forces only. T then corresponds to temperature, and we may, in the application of this formula, disregard the internal motions and forces of the elements of the molecules.

Or (2) we may regard T as the total energy, and $\Sigma\Sigma(\frac{1}{2}Rr)$ as the sum of the virials of all forces whatever. But then T ceases to correspond in the same manner to temperature, and that part of T which represents the internal energy of the molecules is equal to that part of $\Sigma\Sigma(\frac{1}{2}Rr)$ which represents the virials of the internal forces taken negatively.

These latter do not vary with the change of v , or affect $\frac{dU}{dv}$. Here $p_1 = \frac{2}{3} \frac{T_1 + T_2}{v}$, $p_2 = -\frac{dU'}{dv} - \frac{2}{3} \frac{T_2}{v}$, and

$$p = \frac{2}{3} \frac{T_1}{v} - \frac{dU'}{dv}, \quad . \quad . \quad . \quad . \quad . \quad (8)$$

where T_1 is the kinetic energy of the molecules, and T_2 their internal energy, and $\frac{dU'}{dv} dv$ is the hypothetical change of potential of the intermolecular forces only—the same result as we should have obtained by neglecting altogether the effect of the latter portion of T , and also the internal forces of the molecules.

Now it has been shown by Boltzmann that if, in such a gas, the number of variables in each molecule be n , the total energy T is to the energy of translation T_1 as n to 3, or

$$T_1 = \frac{3T}{n}.$$

We have therefore

$$p dv = \frac{2T}{n} \frac{dv}{v} - \frac{dU'}{dv} dv, \quad . \quad . \quad . \quad . \quad . \quad (9)$$

and

$$\frac{dQ}{T} = d \log T + \frac{2}{n} d \log v - \frac{d}{dT} \frac{dU'}{dv} dv + \frac{dU}{dT} d \log T. \quad . \quad (10)$$

Or, observing that T_1 and not T is here the measure of temperature, we may write

$$\frac{dQ}{T_1} = \frac{n}{3} d \log T_1 + \frac{2}{3} d \log v - \frac{d}{dT_1} \frac{dU'}{dv} dv + \frac{dU}{dT_1} d \log T_1, \quad (11)$$

the condition necessary to make $\frac{dQ}{T}$ or $\frac{dQ}{dT_1}$ an exact differential remaining the same as in the case first examined, as expressed in equation (5), except that $\frac{dU'}{dv} dv$ is not the variation of the total potential upon the hypothesis of uniform expansion at any instant, but only such variation of the potential of the forces between the molecules.

XLV. On some Problems connected with the Flow of Electricity in a Plane. By OLIVER J. LODGE, B.Sc.*

THE following paper may be regarded as a sort of appendix to a paper by Professor G. C. Foster and myself, entitled "On the Flow of Electricity in a uniform plane Conducting Surface," and published in the *Philosophical Magazine* for May, June, and December 1875 ([IV.] vol. xlix. pp. 385-400 and 453-471, and vol. l. pp. 475-489), in which the known laws of the flow of electricity in an unlimited plane were deduced from the fundamental idea that the effects of any number of poles could be obtained by simple summation of the effects due to each pole separately, without using any mathematics higher than elementary algebra. Several questions arose in the course of the paper which it was not thought advisable to consider then, but some of which seemed to deserve a separate treat-

* Read before the Physical Society on the 26th of February and the 25th of March, 1876. Communicated by the Society.

ment; these are the subjects I propose to develop in the present communication.

§ 1. Imagine a plane sheet, some portions having infinite conducting-power, other portions consisting of ordinary matter, and again other portions possessing infinite resisting-power. The whole extent of any of the infinitely conducting regions will have a uniform potential; and hence the bounding line between any such region and the ordinary matter of the sheet must be an equipotential line. Further, if one defines a line of flow or stream-line as a line across which no electricity passes, the boundary line between the ordinary matter of the sheet and an infinitely resisting region must be a line of flow, for no electricity can possibly get across it. Such a sheet will therefore contain a certain number of flow-lines and a certain number of equipotential lines. Moreover let the potential of any one of the conducting regions be maintained higher than that of some other one, then in general a uniform flow of electricity will take place throughout the whole of the ordinary matter of the sheet, which will therefore contain additional stream-lines and equipotential lines, whose arrangement will depend on the situations and potentials of the various non-material regions.

Some general properties of the equipotential and stream-lines were pointed out in § 35 of the paper referred to. One of these properties is, of course, that the lines of one system cut the lines of the other orthogonally; a more general statement is the following. If a flow-line passes n times through a given point of the sheet, it will form at that point an equiangular pencil whose $2n$ rays meet each other at an angle $\frac{\pi}{n}$; and if no pole exists at the meeting-point it will be a point of no flow, and an equipotential line will also pass n times through the same point, making an equiangular pencil the same in all respects as the former, but turned through an angle of $\frac{\pi}{2n}$, so that its rays bisect the angles of the first pencil. The ordinary orthogonal section is the special case of this when $n=1$. The special case $n=\infty$ is obtained if, in the sheet imagined above, one of the conducting regions touches one of the resisting regions along a line; when this happens, the line separating the two may be regarded indifferently as a stream-line or as an equipotential line.

There are then two ways of making any given line a stream-line—viz. either by arranging the non-conducting regions so that the line shall bound one of them, or else by arranging the conducting regions and their potentials in such a way that none of the flow of electricity shall take place *across* any por-

tion of the given line. Similarly any line may be made an equipotential line, either by making it bound a region of infinite conductivity, or else by adjusting the potentials and positions of these same regions so that no part of the flow shall take place *along* the given line.

Such a sheet as we have imagined can be approximated to by a piece of tinfoil cut to any shape, with any number of holes in it, and having on its surface blocks of clean copper which are connected with a thermo-pile or other means of maintaining constant difference of potential.

§ 2. The conditions of flow in such a sheet, however, are in general unknown. But if infinitely resisting regions are absent, and if the infinitely conducting regions are reduced to mere points of infinite potential, we get the case of an unlimited sheet containing point poles; and in this all the conditions of the flow are completely known for every point of the sheet. Thus the potential at any point distant $r_1, r_2 \dots$ from the poles which emit quantities of electricity $q_1, q_2 \dots$ in unit time, and which tend to produce potentials $\phi_1, \phi_2 \dots$ at unit distance from themselves, is (§ 33 of the former paper)

$$V = \Sigma \phi - \frac{1}{2\pi\kappa\delta} \Sigma q \log r, \dots \dots \dots (\text{A})$$

where δ is the thickness and κ the specific conductivity of the sheet; and from this expression stream-line equations and resistance expressions follow.

So, then, the problem of finding the flow conditions in any bounded sheet containing point poles will be solved if we can imitate the electrical boundary conditions in an unlimited sheet by introducing extra point poles into it. These additional poles are called images in the boundary, because they produce the same effect in the given portion of the sheet as the boundaries themselves produced—just as the illumination inside a mirror-walled room containing candles would be imitated in unlimited space by placing extra candles at all the points occupied by the images of the original candles in the mirrors.

Images in rectilinear boundaries.

§ 3. Now let it be required to cause a given *straight* line in an infinite sheet of tinfoil to be a stream-line. All that is necessary is to arrange all the blocks of copper in the sheet symmetrically with respect to this line; that is to say, they must either be placed symmetrically upon the line, or they must be placed in pairs one on each side of it and at equal distances from it. It is usual to express this fact in the inverse way thus:—The image of a pole in a straight flow-line is a pole of

the same size, shape, and strength, and situated at an equal distance on the other side of the line so as to be opposite to the first pole.

Again, in order to make a given straight line into an equipotential line, the blocks must be arranged skew-symmetrically with respect to the line—that is to say, in pairs one on each side of it and at equal distances from it, but the sign of one of the constituents of each pair must be opposite to that of the other. This fact is expressed thus :—The image of a pole in a straight equipotential line is a pole of the same size, shape, and numerical strength, but of opposite sign, and situated at an equal distance on the other side of the line.

Hence, if we have given a sheet of tinfoil bounded by a straight edge (*i. e.* by a straight flow-line) and with any distribution of poles in it whatever, and we wish to find the distribution of poles which would make that straight line a flow-line in the unlimited sheet, we have only to regard the line as a plane mirror whose silvered face is turned towards the sheet, and the optical images of the several poles will give the distribution required. The distribution of poles will be obtained in the same way if the boundary is a straight band of copper (*i. e.* an equipotential line); but in this case the sign of every image must be reversed.

§ 4. Now take a sheet bounded by two straight flow-lines intersecting one another at any angle θ : every pole in such a sheet will be reflected backwards and forwards between the two edges according to optical laws; and hence it should have a finite series of images lying on a circle and agreeing with those in the kaleidoscope, the reflection coming to an end as soon as a couple of images fall in the space which is behind both mirrors. But here arises a difficulty. When θ is a submultiple of π , the last couple of images coincide and the whole set is symmetrical with respect to both the straight lines, as is required. But if θ is not a submultiple of π , all that happens in the case of the kaleidoscope is that the last two images separate, the apparent number of images being simply increased by 1. But now the images are not arranged symmetrically with respect to *both* the straight lines, and hence they cannot both be flow-lines. In order to obtain symmetry, it is necessary to continue the reflecting process as if the back of the mirrors were silvered as well as the front; and in that case, of course, the number of images becomes infinite. But then some of these infinite images will fall inside the angle θ (that is, will come into the sheet itself), which will be inconsistent with the given distribution of poles in the sheet. Hence it appears to be impossible for the uniform infinite sheet to contain two

straight flow-lines which enclose any angle but a submultiple of π , unless the poles are put infinitely close together, forming a complete circular ring whose centre is at the intersection of the two lines; and in this case *every* straight line drawn in the sheet from this centre is a flow-line.

Another way of expressing the facts is to say that both sides of *every* flow-line are to be regarded as mirrors, and its images as always infinite in number though often coincident with one another, and that the problem of reducing the bounded case to that of the infinite sheet becomes impossible whenever the images reflected back into the sheet itself do not coincide both in sign and position with the images already there. Similar statements are true of the images in a pair of intersecting equipotential lines.

Another distinction between optical and electrical reflection is, that when two optical images coincide their light-emitting powers are added, but, in the electrical case, coincidence of one image with another does not increase its strength. These differences appear to forbid our imagining that there is any physical meaning in electrical reflection at boundaries analogous to the physical reflection of the waves of light in a mirror.

§ 5. Consider now the images of a point inside a polygon of n sides. We have seen that, without silvering both faces of the boundaries and thereby introducing an unconscionable number of images, some of them *real*, we can only treat angles that are submultiples of π by the method of images. Hence obtuse angles less than π are not allowable. This condition at once excludes all polygons of more than four sides; for as the largest interior angle of a polygon of n sides cannot be less than $\frac{n-2}{n}\pi$, and as it must be less than or equal to $\frac{\pi}{2}$, it is necessary that n shall be

equal to or less than 4. And if $n=4$, the polygon must be equiangular, that is, must be a rectangle. If $n=3$, the triangles whose angles are submultiples of π are the equiangular, the isosceles right angled, and the one whose angles are 90° , 60° , and 30° . The pseudo-triangle whose angles are 90° , 90° , and 0° is also a possible case, and is treated below as a special case of the circular sector.

I have not mentioned the case when π itself is one of the angles of a polygon, because this merely reduces the polygon to one of $n-1$ sides; but there is the case of the equiangular polygon with all its angles π , viz. the circle, about which a little shall be said later.

The images of a point in a rectangle occur in groups of four surrounding the vertices of rectangles whose linear dimensions

are twice those of the original one, and which cover all the plane.

The images of a point in an equilateral triangle occur in groups of six surrounding the vertices of triangles $\sqrt{3}$ times the linear dimensions of the original one, and covering all the plane.

Application of images to certain cases of bounded plates.

§ 6. Now although the potential of any point is really determined by (A), § 2, as soon as we know the positions of all the images in the sheet, the expression is not in a practicable form; and it would not in the majority of cases be readily possible to bring it to such a form. The way to proceed in such cases is to consider what is required of the potential, and then to write down a function of $x + iy$,

$$W \equiv V + Ui,$$

such that the real part satisfies all the conditions required of the potential function, viz. to remain together with its derivatives continuous and finite all over the sheet except at the poles and their images, where it is to become infinite like $q \log r$, to fulfil certain given conditions at the boundary, and to satisfy the equation

$$\frac{d^2V}{dx^2} + \frac{d^2V}{dy^2} = 0.$$

This being done, $V = \text{const.}$ will represent the equipotential lines, and $U = \text{const.}$ the stream-lines, and the components $\frac{dV}{dx}$, $\frac{dV}{dy}$ of the flow at any point can be obtained. In this way Dr. E. Jochmann* has expressed all the conditions of the flow in a *rectangle*, bounded either by stream-lines or by equipotential lines, or by both.

§ 7. But though the potential expression (A) is often of impracticable form, the expression for the resistance of a conductor, which follows from it, is simpler, and admits of being reduced to a useful form with much ease in many cases. Expressions so obtained may serve to verify the results of more abstruse and general processes; but the principal reason for entering into the subject is that the expressions themselves and several of the continued products which lead to them seem to be of some little mathematical interest.

* "Ueber einige Aufgaben, welche die Theorie des logarithmischen Potentials und den Durchgang eines constanten elektrischen Stroms durch eine Ebene betreffen," von Dr. E. Jochmann in Berlin. Schlömilch, *Zeitschrift für Mathematik*, 1865, p. 48.

Resistance of some rectilinear figures to the current flowing between two small circular electrodes.

§ 8. Let us first remind ourselves how the general resistance-expression is obtained. Given an infinite sheet with any number k of equal sources $A_1, A_2, \dots A_k$ and the same number of equal sinks $B_1, B_2, \dots B_k$ in it, the resistance between the two equipotential lines which pass through any two fixed points P and Q is given by Ohm's law as

$$R_{PQ} = \frac{V_P - V_Q}{kq},$$

where V_P stands for the actual potential at the point P , and kq stands for the whole strength of current flowing in the sheet—that is to say, for the quantity of electricity emitted by each source per second, multiplied by the number of sources. Now (A) gives as the potential at any point P

$$V_P = -\frac{q}{2\pi\kappa\delta} \log \frac{A_1P \cdot A_2P \dots}{B_1P \cdot B_2P \dots}, \quad \dots \dots \dots (A)$$

hence

$$R_{PQ} = \frac{1}{2k\pi\kappa\delta} \log \left\{ \frac{A_1Q \cdot A_2Q \dots}{B_1Q \cdot B_2Q \dots} \cdot \frac{B_1P \cdot B_2P \dots}{A_1P \cdot A_2P \dots} \right\}. \quad \dots (B)$$

And these expressions are perfectly general; for unequal poles may be regarded as aggregations of equal ones, and the number of sinks *must always* be equal to the number of sources, although some of them may often be at infinity.

Now we know that the equipotential lines of high potential break up into k portions which ultimately become little circles, one surrounding each source. Similarly the lines of low potential surround the sinks. Take the point P on the circumference of one of these little circles (radius ρ_1), that surrounding the source A say (omitting its suffix); and take the point Q on a circle (radius ρ_2) surrounding the sink B . Then the resistance between the two small circular electrodes A and B is k times as much as that offered by the sheet to the $2k$ poles, or

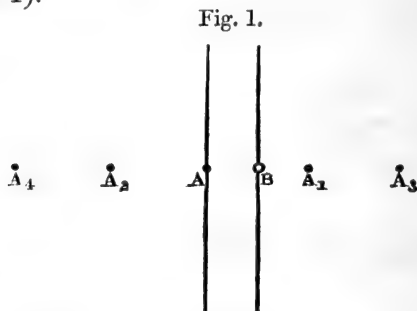
$$R_{AB} = \frac{1}{2\pi\kappa\delta} \log \left\{ \frac{AB \cdot A_1B \cdot A_2B \dots}{\rho_1 \cdot A_1A \cdot A_2A \dots} \cdot \frac{BA \cdot B_1A \cdot B_2A \dots}{\rho_2 \cdot B_1B \cdot B_2B \dots} \right\}, (\alpha)$$

where the higher powers of ρ_1 and ρ_2 are neglected. Since they are both small there is no necessity to distinguish them, and ρ^2 may be written for their product. Moreover in very many cases the sources and sinks will be similarly arranged; so that the two fractions which are multiplied together inside the brackets of (α) are equal to one another, and we may then use

the simplified expression

$$R_{AB} = \frac{1}{\pi\kappa\delta} \log \left(\frac{AB \cdot A_1B \cdot A_2B \dots}{\rho \cdot A_1A \cdot A_2A \dots} \right). \quad (\beta)$$

§ 9. Before proceeding to apply the image theory and this expression to the general case of a two-sided figure, it will be well to consider a special case or two,—and first the very simple case of a parallel-sided strip of breadth s , with the two poles, A and B, one on each side of the strip and opposite to one another (fig. 1).



A will be reflected in the opposite side, its image A_1 being at a distance s from that side and from B; then A_1 will be reflected, its image A_2 being $2s$ distant from A and $3s$ from B; then A_2 will be reflected, and so on. The images of B will evidently be similarly arranged to those of A *mutatis mutandis*; so we may use the second of the resistance-formulæ

$$R = \frac{2}{\pi\kappa\delta} \log \frac{AB \cdot A_1B \dots}{\rho \cdot A_1A \dots},$$

doubling it because the poles are only halfway on the sheet (see below, § 22). Now

$$\begin{aligned} AB &= A_1B = s, & A_1A &= A_2A = 2s, \\ A_2B &= A_3B = 3s, & A_3A &= A_4A = 4s, \\ A_4B &= A_5B = 5s, & & \dots \dots \dots \\ & & & \dots \dots \dots \end{aligned}$$

Hence the above product becomes

$$\frac{s}{\rho} \cdot \frac{1 \cdot 3 \cdot 3 \cdot 5 \cdot 5 \cdot 7 \dots}{2 \cdot 2 \cdot 4 \cdot 4 \cdot 6 \cdot 6 \dots},$$

which equals $\frac{s}{\rho} \cdot \frac{2}{\pi}$.

Therefore

$$R = \frac{2}{\pi\kappa\delta} \log \frac{2s}{\pi\rho}. \quad \dots \dots \dots (1)$$

§ 10. One more special case of the infinite strip I will take, because a constantly occurring product occurs here in its simplest form. Let the two poles be both on the same side of the strip and at a distance c from one another. The images of A are just as before, and the product $\frac{AB}{\rho} \cdot \frac{A_1B}{A_1A} \cdot \frac{A_2B}{A_2A} \dots$ equals

$$\left\{ \frac{c^2}{\rho^2} \cdot \frac{2^2 s^2 + c^2}{2^2 s^2} \cdot \frac{2^2 s^2 + c^2}{2^2 s^2} \cdot \frac{4^2 s^2 + c^2}{4^2 s^2} \cdot \frac{4^2 s^2 + c^2}{4^2 s^2} \dots \right\}^{\frac{1}{2}}$$

$$= \frac{c}{\rho} \left(1 + \frac{c^2}{2^2 s^2} \right) \left(1 + \frac{c^2}{4^2 s^2} \right) \left(1 + \frac{c^2}{6^2 s^2} \right) \dots$$

$$= \frac{c}{\rho} \cdot \frac{\sinh \frac{\pi c}{2s}}{\frac{\pi c}{2s}} *.$$

Therefore

$$R = \frac{2}{\pi \kappa \delta} \log \left(\frac{2s}{\pi \rho} \sinh \frac{\pi c}{2s} \right). \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Similarly, if we had taken the poles one on each side of the strip, but, instead of opposite each other as at first, at a distance c from one another measured parallel to the sides of the strip, we should have obtained

$$R = \frac{2}{\pi \kappa \delta} \log \left(\frac{2s}{\pi \rho} \cosh \frac{\pi c}{2s} \right). \quad . \quad . \quad . \quad . \quad . \quad (3)$$

If s be made infinite, (2) becomes $\frac{2}{\pi \kappa \delta} \log \frac{c}{\rho}$, which is the correct expression for the case of two poles on the edge of half an infinite sheet. If s is small, (2) and (3) become equal;

* Here and elsewhere $\sinh x$ may be considered merely an abbreviation for $\frac{e^x - e^{-x}}{2}$, or as equal to $\frac{1}{i} \sin ix$. Similarly

$$\cosh x = \frac{e^x + e^{-x}}{2}, \text{ or } = \cos ix,$$

and

$$\tanh x = \frac{e^x - e^{-x}}{e^x + e^{-x}} = \frac{1}{i} \tan ix,$$

where $i = \sqrt{-1}$. It is useful to remember that

$$\cosh^2 x - \sinh^2 x = 1,$$

that

$$\sinh 0 = \tanh 0 = \sin 0 = 0,$$

that

$$\cosh 0 = \tanh \infty = 1,$$

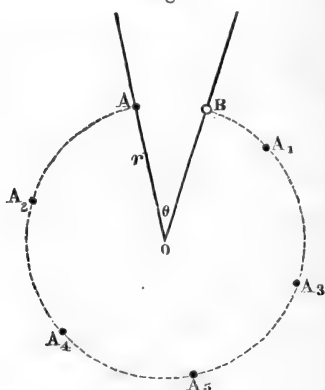
and that

$$\sinh \infty = \cosh \infty = \frac{1}{2} e^\infty$$

and if $s=\rho$, they reduce to $R=\frac{c}{\kappa\rho\delta}$ (neglecting the small term $-\frac{2}{\pi\kappa\delta}\log\pi$), which is right for the resistance of a wire or thin strip of sectional area $\rho\delta$ and length c . The presence of the term neglected in comparison with a term which has ρ in its denominator, can be accounted for by remembering that the electrodes are not exactly straight bars at a distance c , but quadrants of small circles whose centres are distant c from one another.

§ 11. We may also consider one special case of the infinite "wedge" or *irregular* two-sided polygon. Let the two poles be one on each side of the wedge at the same distance r from its angle O, and call this angle $\theta=\frac{\pi}{n}$ (n integer). The images of A will lie on a circle with centre O (fig. 2), as in the kaleidoscope.

Fig. 2.



Then

$$AB=A_1B=2r\sin\frac{\theta}{2}, \quad A_1A=A_2A=2r\sin\frac{2\theta}{2},$$

$$A_2B=A_3B=2r\sin\frac{3\theta}{2}, \quad A_3A=A_4A=2r\sin\frac{4\theta}{2},$$

..... ;

so the product which occurs in (β) equals

$$\frac{2r}{\rho} \cdot \frac{\sin^2\frac{\theta}{2} \sin^2\frac{3\theta}{2} \sin^2\frac{5\theta}{2} \dots}{\sin^2\frac{2\theta}{2} \sin^2\frac{4\theta}{2} \sin^2\frac{6\theta}{2} \dots} \begin{cases} \frac{\sin^2\frac{n-1}{2}\theta}{\sin^2\frac{n}{2}\theta} \sin\frac{n\theta}{2} & \text{when } n \text{ is even,} \\ \frac{\sin^2\frac{n-2}{2}\theta}{\sin^2\frac{n-1}{2}\theta} \sin\frac{n\theta}{2} & \text{when } n \text{ is odd.} \end{cases}$$

But we know from Trigonometry that

$$\sin \frac{\pi}{2n} \sin \frac{3\pi}{2n} \dots \left\{ \begin{array}{l} \sin \frac{n-1}{2n} \pi \\ \sin \frac{n-2}{2n} \pi \end{array} \right. = 2^{\frac{2}{n-1}},$$

and that

$$\sin \frac{2\pi}{2n} \sin \frac{4\pi}{2n} \dots \left\{ \begin{array}{l} \sin \frac{n-2}{2n} \pi \\ \sin \frac{n-1}{2n} \pi \end{array} \right. = 2^{\frac{2}{n-1}} \sqrt{n},$$

the upper termination to be taken when n is even, the lower when n is odd. Hence the above product equals

$$\frac{2r}{\rho} \cdot \frac{1}{n},$$

and

$$R = \frac{2}{\pi \kappa \delta} \log \frac{2r\theta}{\pi \rho} \text{ or } = \frac{2}{\pi \kappa \delta} \log \frac{2s}{\pi \rho}, \quad . \quad . \quad . \quad (4)$$

if one writes s for the arc subtending θ of the circle whose radius is r .

Notice here the close resemblance to (1), becoming identity when $r = \infty$, and also the sort of circular analogy to Wallis's form of $\frac{\pi}{2}$ which one gets by taking arcual instead of linear distances in fig. 2 for the product of (β) .

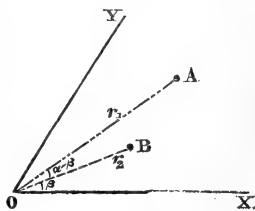
The circle on which all the poles lie is a flow-line by symmetry; and hence the resistance of the sector A O B is twice that of the wedge.

General resistance of a "Wedge," or two-sided polygon, poles any where.

§ 12. If we obtain an expression for the case of two poles A and B, it will be little more than a matter of writing to extend it to any number. Let O X and O Y be the two sides of the wedge, the angle Y O X being $\theta = \frac{\pi}{n}$. Call the distance O A,

r_1 ; and the angle A O X, α (fig. 3); and let (r_2, β) denote the point B. The images of A will lie in pairs on a circle of radius r_1 , those of B on a circle of radius r_2 ; and to find the value of the products which occur in the general expression (α) we may make use of Cotes's (or Demoivre's) property of the circle, which says that, if m points equisect the circumference of a circle of radius r_1 , the product of their distances from an-

Fig. 3.



other point, distant r_2 from the centre of the circle and $\sqrt{(r_1^2 - 2r_1r_2 \cos \phi + r_2^2)}$ from the nearest of the m points, will be equal to

$$\sqrt{(r_1^{2m} - 2r_1^m r_2^m \cos m\phi + r_2^{2m})}. \quad \dots \quad (C)$$

To apply this theorem to the present case we must consider the images of A in two sets:—one set including A itself and all its images whose angular distance from it is some multiple of 2θ ; the other set containing all the remaining images, which will alternate with those of the first set, and will like them divide the circle into n equal parts. The product of all the distances of the first set from the point B will be given by (C) if we put

$m = n = \frac{\pi}{\theta}$ and $\phi = \alpha - \beta$; the product for the second set will

only differ in having $\phi = \alpha + \beta$. From the symmetry of the expression, it follows that the product of the distances of the images of B from the point A will be the same as that of the images of A from the point B. Hence the numerator of (α) is determined. As for the denominator, the product of the distances of the second set of images of A from the point A is

$$2r_1^n \sin n\alpha.$$

The product of the first set appears to be zero, since A itself is one of the set; but remembering that the point to which distances are properly measured is not the actual pole A but a point on the circumference of its small electrode (§ 8), we see that the product becomes

$$r_1^n - (r_1 - \rho)^n = nr_1^{n-1}\rho,$$

neglecting high powers of ρ . So the whole product of the A's from A is $2n\rho r_1^{2n-1} \sin n\alpha$; similarly the product of the B's from B will be

$$2n\rho r_2^{2n-1} \sin n\beta.$$

Hence the resistance-expression (α) reduces to

$$\Re = \frac{1}{2\pi\kappa\delta} \log$$

$$\frac{(r_1^{2n} - 2r_1^n r_2^n \cos n(\alpha - \beta) + r_2^{2n})(r_1^{2n} - 2r_1^n r_2^n \cos n(\alpha + \beta) + r_2^{2n})}{4n^2 \rho^2 r_1^{2n-1} r_2^{2n-1} \sin n\alpha \cdot \sin n\beta}, \quad (5)$$

or, as it is often more convenient to write it,

$$= \frac{1}{2\pi\kappa\delta} \log \left\{ \frac{r_1 r_2}{\rho^2} \cdot \frac{\left(\left[\frac{r_1}{r_2} \right]^n + \left[\frac{r_2}{r_1} \right]^n - 2 \cos n\overline{\alpha - \beta} \right) \left(\left[\frac{r_1}{r_2} \right]^n + \left[\frac{r_2}{r_1} \right]^n - 2 \cos n\overline{\alpha + \beta} \right)}{4n^2 \sin n\alpha \cdot \sin n\beta} \right\}$$

§ 13. A few special cases of this may be recorded.

When $r_1=r_2$, it becomes

$$\frac{1}{\pi\kappa\delta} \log \left(\frac{r}{\rho} \cdot \frac{\cos n\alpha - \cos n\beta}{n \sqrt{(\sin n\alpha \cdot \sin n\beta)}} \right). \quad . \quad . \quad . \quad (6)$$

If at the same time α be made zero and $\beta=\theta$, we ought to get (4); but we must remember that α is not actually to vanish, it is to become very small and equal to $\frac{\rho}{2r}$, which is a sort of compromise between $\frac{\rho}{r}$, the greatest value it could vanish with, and $\frac{0}{r}$, the least. So putting

$$\alpha = \frac{\rho}{2r}, \quad \beta = \theta - \frac{\rho}{2r},$$

(6) becomes

$$\frac{1}{\pi\kappa\delta} \log \left(\frac{r}{n\rho} \cdot \frac{2}{\frac{n\rho}{2r}} \right) = \frac{2}{\pi\kappa\delta} \log \frac{2r}{n\rho},$$

which agrees with (4).

Going back to r_1 and r_2 unequal, let us put both poles on OX—that is, write

$$\alpha=0=\frac{\rho}{2r_1} \text{ and } \beta=0=\frac{\rho}{2r_2},$$

(5) will become

$$\frac{1}{\pi\kappa\delta} \log \left(\frac{r_1^n - r_2^n}{n^2 \rho^2 r_1^{n-1} r_2^{n-1}} \right)^2; \quad . \quad . \quad . \quad . \quad (7)$$

and this reduces to (2) when r is made infinite.

Similarly if the poles were placed one on OX and the other on OY, we should get

$$\frac{1}{\pi\kappa\delta} \log \frac{(r_1^n + r_2^n)^2}{n^2 \rho^2 r_1^{n-1} r_2^{n-1}}; \quad . \quad . \quad . \quad . \quad (8)$$

and this reduces to (3) when $r_1=r_2+c=\infty$, and $\theta=\frac{s}{r}=0$.

One more case is worth mentioning, viz. when one of the poles, say B, is on the angle of the wedge at O. To give this, $r_2=\rho$ and (β being any thing between 0 and θ , $n\beta$ will be any thing between 0 and π , say $\frac{\pi}{2}$) $\sin n\beta=1$, so

$$\Re = \frac{1}{2\pi\kappa\delta} \log \left(\frac{r_1^{2n+1}}{\rho^{2n+1}} \cdot \frac{1}{2n \sin n\alpha} \right). \quad . \quad . \quad (9)$$

If at the same time A is on OX or OY, this becomes

$$\frac{n+1}{\pi\kappa\delta} \log \left\{ \frac{r_1}{\rho} \cdot \left(\frac{\theta}{\pi} \right)^{\frac{1}{n+1}} \right\}. \quad \dots \quad (10)'$$

A direct application of Cotes's property and equation (B) to this last case gives us as the perfectly accurate expression

$$\frac{n}{\pi\kappa\delta} \log \left\{ \frac{r_1 - \rho_1}{\rho_2} \left(\frac{r_1^n - \rho_2^n}{r_1^n - (r_1 - \rho_1)^n} \right)^{\frac{1}{n}} \right\}, \quad \dots \quad (10)$$

whatever be the size of the electrodes, provided that they be taken of such shape as to fit the equipotential lines passing through the two points P and Q to which distances are measured. Both these points are on the line AB, one of them distant ρ_1 from A, the other ρ_2 from B. This expression will reduce to (10)' when ρ_1 and ρ_2 are regarded as small quantities of the first order.

§ 14. Although all these expressions are proved only when θ is a submultiple of π , yet by continuity they would seem likely to hold true always. If so, we have expressed the resistance of a Riemann's surface of any number of leaves, but all connected at one branch-point O.

General resistance of a "Strip," or regular two-sided polygon poles anywhere. Fig. 4.

§ 15. The strip being a special case of the wedge, its resistance will follow directly from (5)* by making θ vanish. We must substitute

$$s = r_1\theta, \quad a = r_1\alpha, \quad b = r_2\beta, \quad c = r_1 - r_2,$$

and then make $r_1 = r_2 + c$ infinite, keeping c, a, b, s all finite (fig. 4). The substitutions give us

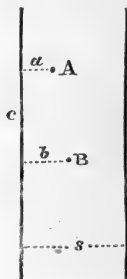
$$\alpha = \frac{a}{s}\theta, \quad \beta = \frac{b}{s} \frac{r_1}{r_2} \theta, \quad n = \frac{\pi}{\theta} = \frac{\pi r_1}{s};$$

so the resistance (5)' becomes

$$\Re = \frac{1}{2\pi\kappa\delta} \log$$

$$\left\{ \frac{s^2}{\pi^2 \rho^2} \cdot \frac{\left(\cosh \frac{c}{s} \pi - \cos \frac{a-b}{s} \pi \right) \left(\cosh \frac{c}{s} \pi - \cos \frac{a+b}{s} \pi \right)}{\sin \frac{a\pi}{s} \sin \frac{b\pi}{s}} \right\}. \quad (11)$$

* It might be objected that when n is made large, higher powers of ρ in the expansion of $(r - \rho)^n$ may not be neglected, as was done in the process of obtaining (5). This would be true if the size of n were merely due to the shutting up of θ with a constant r ; but in our present case r is also large, and the expansion is in descending powers of r .



§ 16. For the case when both poles are at the same distance from the sides of the strip, or $a=b$, this becomes

$$\frac{1}{\pi\kappa\delta} \log \left\{ \frac{2s}{\pi\rho} \sinh \frac{\pi c}{2s} \left(\frac{\cosh \frac{c\pi}{s} - \cos \frac{2a\pi}{s}}{1 - \cos \frac{2a\pi}{s}} \right)^{\frac{1}{2}} \right\}; \quad (12)$$

and if they are also both in the middle of the strip, it simplifies to

$$\frac{1}{\pi\kappa\delta} \log \left(\frac{s}{\pi\rho} \sinh \frac{\pi c}{s} \right), \quad \dots \dots \dots (2)'$$

which is just half (2), as it ought to be. I write out this particularly, because this case has been already studied by Stefan, and experimentally verified by von Obermayr (*Wien. Akad. Ber.* 1869, vol. lx. part 2. p. 245). Stefan, it seems, wrote the expression he obtained thus (merely altering his letters),

$$\frac{1}{\pi\kappa\delta} \log \frac{s}{2\pi\rho} + \frac{1}{\pi\kappa\delta} \log (e^{\frac{\pi c}{s}} - e^{-\frac{\pi c}{s}});$$

and this is identical with (2)'.

§ 17. It seems as if expressions like the above might possibly be employed in obtaining the values of certain continued products. Thus to take (12) as an example.

The direct geometrical product for this case, occurring in (β), is the root of

$$\frac{c^2}{\rho^2} \cdot \frac{2a}{2s-2a} \cdot \frac{2s}{2s} \cdot \frac{2s}{2s+2a} \cdot \frac{4s}{4s-2a} \cdot \frac{4s}{4s} \cdot \frac{4s}{4s+2a} \cdot \frac{6s}{6s-2a} \dots,$$

where $\frac{ks \pm 2a}{c^2}$ is an abbreviation for $1 + \frac{c^2}{(ks \pm 2a)^2}$.

Now one portion of this we know (cf. § 10),

$$\left\{ \left(1 + \frac{c^2}{2^2 s^2} \right) \left(1 + \frac{c^2}{4^2 s^2} \right) \left(1 + \frac{c^2}{6^2 s^2} \right) \dots \right\}^2 = \left(\frac{2s}{\pi c} \sinh \frac{\pi c}{2s} \right)^2 = A^2 \text{ say;}$$

and as $A^2 \frac{c^2}{\rho^2}$ equals the square of the rational part of the quantity inside the brackets of (12), it follows that the square of the irrational part is equal to the remaining factors of the above product. It may be written

$$\frac{\cosh \frac{2c\pi}{s} - \cos \frac{2a\pi}{s}}{1 - \cos \frac{2a\pi}{s}} = 1 + \frac{\sinh^2 \frac{c\pi}{s}}{\sin^2 \frac{a\pi}{s}} = \left(1 + \frac{c^2}{a^2} \right) \left(1 + \frac{c^2}{(s-a)^2} \right)$$

$$\left(1 + \frac{c^2}{(s+a)^2} \right) \left(1 + \frac{c^2}{(2s-a)^2} \right) \left(1 + \frac{c^2}{(2s+a)^2} \right) \dots,$$

merely putting s instead of $2s$, and a instead of $2a$.

Whether this result is otherwise obvious or not, it will serve as an example of how new products might sometimes be evaluated from resistance-expressions.

General resistance of a circular sector.

§ 18. The resistance of a circular sector containing two poles A and B will be twice the resistance of a wedge with the same angle, but in which, besides the poles A and B, there also exist poles A' and B' at the points inverse to A and B with regard to the circle of which the sector is a part; that is, calling R the radius of the sector, $OA' = \frac{R^2}{r_1}$, $OB' = \frac{R^2}{r_2}$. The expression for the resistance will be (5) plus an additional expression

$$\Re = (5) + \frac{1}{2\pi\kappa\delta} \log$$

$$\left\{ \left| \frac{R^2}{r_1 r_2} \right|^n + \left| \frac{r_1 r_2}{R^2} \right|^n - 2 \cos n\alpha - \beta \right\} \left\{ \left| \frac{R^2}{r_1 r_2} \right|^n + \left| \frac{r_1 r_2}{R^2} \right|^n - 2 \cos n\alpha + \beta \right\}$$

$$\left(\left| \frac{R}{r_1} \right|^n - \left| \frac{r_1}{R} \right|^n \right) \left(\left| \frac{R}{r_2} \right|^n - \left| \frac{r_2}{R} \right|^n \right) \left(\left| \frac{R}{r_1} \right|^{2n} + \left| \frac{r_1}{R} \right|^{2n} - 2 \cos 2n\alpha \right)^{\frac{1}{2}} \left(\left| \frac{R}{r_2} \right|^{2n} + \left| \frac{r_2}{R} \right|^{2n} - 2 \cos 2n\beta \right)^{\frac{1}{2}}$$

§ 19. When the angle of the sector vanishes by R becoming infinite, we get the case of a strip bounded towards one end, or what may be called an isosceles triangle whose equal angles are $\frac{\pi}{2}$. The pole A being at a distance c_1 from the base and a from one of the sides, while B is at a distance b from the same side, c_2 from the base, the above expression reduces to

$$R = (11) + \frac{1}{2\pi\kappa\delta} \log$$

$$\left(\cosh \frac{c_1 + c_2}{s} \pi - \cos \frac{a - b}{s} \pi \right) \left(\cosh \frac{c_1 + c_2}{s} \pi - \cos \frac{a + b}{s} \pi \right)$$

$$2 \sinh \frac{\pi c_1}{s} \sinh \frac{\pi c_2}{s} \sqrt{\left(\cosh \frac{2\pi c_1}{s} - \cos \frac{2a\pi}{s} \right)} \sqrt{\left(\cosh \frac{2\pi c_2}{s} - \cos \frac{2b\pi}{s} \right)}$$

where the c in (11) may be written $c_1 - c_2$ for symmetry.

It may be noticed that the additional quantity in (14) vanishes when c_1 and c_2 are infinite; but becomes the same as (11) when c_1 and c_2 are made small and equal to $\frac{\rho}{2}$; and this is just what it ought to do.

§ 20. The quantity which is added to (11) to give (14) may be called the "effect of the boundary" which limits the strip at one end. If we added a boundary to the other end of the

strip also, it would become a rectangle of breadth s and length $c_1 + c_3$ or $c_2 + c_4$; and by adding another quantity to (11) similar to the above, but with c_3 and c_4 instead of c_1 and c_2 (that is, by adding a quantity which approximates to the effect of the new boundary), we should obtain an approximation to the resistance of a rectangle, which is a pretty close one if the c are larger than the s . Such an approximate expression, however, has no particular interest. It is easy to write down the products occurring in (α) for the general case of a rectangle; but their evaluation does not appear to be so simple.

Distribution of potential in a "wedge" containing any number of point poles.

§ 21. All the flow-conditions in a wedge whose angle is $\frac{\pi}{n}$ are determined completely by writing out the particular form assumed by the general expression (A)', § 8, for this case. The polar coordinates of the sources being (r_1, α) , $(r'_1, \alpha') \dots$, and of the sinks being (r_2, β) , $(r'_2, \beta') \dots$, the potential at any point (r, ϕ) is

$$V = \frac{q}{4\pi\kappa\delta} \log \frac{(r^{2n} - 2r^n r_2^n \cos n\phi - \beta + r_2^{2n})(r^{2n} - 2r^n r_2^n \cos n\phi + \beta + r_2^{2n})}{(r^{2n} - 2r^n r_1^n \cos n\phi - \alpha + r_1^{2n})(r^{2n} - 2r^n r_1^n \cos n\phi + \alpha + r_1^{2n})} \quad (15)$$

+ similar terms with accented coordinates for every other pair of poles which the sheet may contain.

When the wedge becomes a "strip" of breadth s , the above expression gives, as the potential of any point (x, y) ,

$$V = \frac{q}{4\pi\kappa\delta} \log \frac{\left(\cosh \frac{y-y_2}{s} \pi - \cos \frac{x-x_2}{s} \pi \right) \left(\cosh \frac{y-y_2}{s} \pi - \cos \frac{x+x_2}{s} \pi \right)}{\left(\cosh \frac{y-y_1}{s} \pi - \cos \frac{x-x_1}{s} \pi \right) \left(\cosh \frac{y-y_1}{s} \pi - \cos \frac{x+x_1}{s} \pi \right)} \quad (16)$$

+ &c.,

where the sources are (x_1, y_1) , $(x'_1, y'_1) \dots$, the sinks (x_2, y_2) , $(x'_2, y'_2) \dots$, and where one side of the strip is taken as the axis of y .

[To be continued.]

XLVI. *On the Determination of the Longitude of Cairo from Greenwich by the Exchange of Telegraph-Signals. By Captain C. ORDE BROWNE, Chief of Egypt Transit-of-Venus Expedition*.*

EGYPT having been chosen by the Astronomer Royal as the position for a group of stations for the observation of the retarded egress of Venus in transit on Dec. 8, 1874, the question arose as to the feasibility of employing the telegraph connexions between England and Egypt to exchange time-signals, in order to determine the longitude from Greenwich. The Astronomer Royal decided to make trial of this method, if a reliable signal could be obtained through the cable of the Eastern Telegraph Company, when such connexions should be made as would bring the English end of the cable into direct circuit with that at Alexandria.

With a view to ascertaining this, the Astronomer Royal sent me twice to Porthcurnow, in Cornwall, where the cable comes to land, and where the Eastern Telegraph Company have their terminal station, under the direction of Mr. Edward Bull, to whom the Directors had kindly sent instructions empowering him to devote the cable to the purposes of the experiment for as long a time as might be required. It is not too much to say that to Mr. Bull's services in suggesting expedients, and to his determination and perseverance, is to be attributed the success in getting the signals through so great a length of line.

From Porthcurnow there run two cables—one coming to land at Vigo, which is connected by a separate length of cable with Lisbon, the other proceeding direct from Porthcurnow to Lisbon. A single cable runs from Lisbon to Gibraltar, and another from Gibraltar to Malta. From Malta two cables run to Alexandria.

To connect Porthcurnow directly with Alexandria, then, it is necessary to "join up" the cable-ends at Malta, Gibraltar, and Lisbon, and, if the Vigo cable be used, at Vigo also; and in the experiment that eventually took place this was the line employed. The particulars of the cables thus brought into the circuit are given hereafter.

On the occasion of the second preliminary experiment, Mr. Ellis, of the Royal Observatory, and Mr. S. Hunter, of the Transit-of-Venus Expedition, who had been appointed by the Astronomer Royal to conduct the work in concert with the officers of the Telegraph Company at Porthcurnow and at Alexandria respectively, came down to Porthcurnow, and

* Communicated by the Author.

made an exchange of signals through the circuit formed by the cable lines extending from Porthcurnow to Vigo, and so through Lisbon, and back by the direct cable to Porthcurnow. By this means experience was obtained by the observers at the opposite ends of a cable 1734 knots in length; that is to say, more than half the length of the Porthcurnow-and-Alexandria Cable. This was a valuable preliminary experiment, because the observers at the opposite extremities of the cable were sitting side by side, and able to compare and rectify faults together.

The first object of the Porthcurnow experiments was to ascertain the possibility of getting a reliable signal through the great length of cable between Cornwall and Alexandria, which, it may be observed, is not only 638 knots longer than the Transatlantic one, but has also a much smaller section of copper*. Further, it was necessary to decide what arrangement of apparatus to employ, and to take steps to ensure the same conditions being observed at Porthcurnow and Alexandria when the actual experiment took place. It was found when Thomson's reflecting galvanometer was employed in the usual way, that the movement of the beam of light was feeble, but that with an arrangement proposed by Mr. Bull, an amply sufficient length of travel was obtained.

This arrangement consisted in the attachment of the small mirror to the coil of a Thomson's recorder, by which the former was rendered much more sensitive than when used on the ordinary galvanometer by being brought into a strong magnetic field†.

Signals were made with and without condensers. When no condenser was used, the beam came to no fixed zero position, but was subject to continual fluctuation. On the other hand, when a condenser was employed at the sending end, the travel of the beam of light was not sufficient to furnish a reliable signal.

The following was the arrangement finally adopted:—A condenser was employed at the receiving end only, between the line and instrument. The mirror was used on the recorder

* The Porthcurnow-and-Alexandria cable is 3222 knots, and the French cable from Brest to St. Pierre 2584 knots in length. The copper and gutta-percha coatings are respectively 120 lbs. and 175 lbs. for the English, and 400 lbs. both of copper and of gutta-percha for the French cable per knot.

† In the Thomson recorder a coil of very fine wire is suspended vertically between the extremities of a powerful electromagnet depending on the action of a special battery. The coil is brought into the actual line circuit. Hence a slight current through the line causes strong movement of the coil; indeed the object of the arrangement is strength as much as extreme sensitiveness.

in the manner proposed by Mr. Bull—an instrument being lent by him at my request to take out to Alexandria, in order to ensure the same conditions being observed at both ends of the cable.

Mr. Cromwell Varley, F.R.S., was consulted as to the particular character of signal that his experience in the Atlantic exchanges led him to prefer. He recommended the use of the signal made on the “break” of current, rather than that given by the “make,” because in the former case, the battery being put out of circuit, the danger of irregularity due to unequal resistances in the batteries employed at the terminal stations would be avoided. He also advised that the transit of the beam of light across a line about the middle of its stroke, where its rate of motion was greatest, should be observed, rather than its first movement. To do this, he proposed that the length of travel of the beam should be first observed on a scale, and the halfway line marked, the zero-point being brought to the standing position of the beam a little before the time at which each signal should be due; the current to be made for 50 seconds and broken for 10 seconds, the direction of the current being reversed for each successive signal. At the same time Mr. Varley suggested an ingenious way of ascertaining the effect of earth-currents on the signals. For this purpose the transits of the beam of light across lines made at points at $\frac{1}{4}$ and $\frac{3}{4}$ the length of travel were to be observed. The interval of time between these transits being a function of the time of retardation of current, a comparison of results obtained at each end would give the relative effect of earth-currents or other retarding influences. For this, however, it would be necessary that the travel of the beam should be exactly the same in each case, and that the observation made at these two points of the same movement, which was a difficult matter, should be very accurate. Besides this, a considerable number of signals might have to be devoted to this work. Altogether, the Astronomer Royal decided not to attempt such an investigation; but he directed that one hour out of every four should be given to the observation of the start of the beam at the “make” and “break” of currents sent in opposite directions successively every half minute, that is to say, 15 seconds current and 15 seconds break, followed by an opposite current and break of 15 seconds each. This, taken with Mr. Varley’s method, might even give much information as to earth-currents; moreover it offered the advantage of providing a very large number of a class of signals hitherto employed and found reliable and of a simple character. Mr. Varley, however, strongly recommended the adoption of an intermediate station as giving greater accuracy and security,

and as incidentally giving the longitude of another important place in Europe very accurately. He calculated that the retardation of the current would be about 1.25 second between Porthcurnow and Alexandria, a calculation which was proved by the result to be remarkably correct. The idea of the establishment of an intermediate station was negatived as impracticable by the Astronomer Royal.

Mr. Varley kindly placed his artificial line (furnished with condensers, &c., to represent the conditions of a submarine cable of greater or less length) at the disposal of the Astronomer Royal. By means of this apparatus abundant practice was obtained in the observation of a signal similar to that made on the actual cable. When the expedition had gone out to Egypt, and stations had been established with proper telegraph-connexions, the actual exchange of signals took place. Matters were then in the following condition.

The primary station in Egypt was established on the western spur of the desert range of the Mokattam hills, about 560 feet above Cairo. The real object of the experiment was to obtain the longitude of this point from Greenwich. The longitudes of the subsidiary stations at Thebes and Suez, and if necessary Alexandria also, might be readily obtained from the Mokattam station by exchange of signals through land lines.

To compare the Mokattam local time with Greenwich time, it was necessary to exchange land-line signals between these two places with the transmitting stations established, as above noticed, at each end of the cable; consequently Mokattam and Alexandria exchanged signals, while Greenwich and Porthcurnow were similarly engaged. Then took place the exchange of cable-signals between Porthcurnow and Alexandria, which lasted for four hours. Finally Mokattam and Alexandria again exchanged signals.

While the cable-signals were passing a number of star-transits were observed at Mokattam, as well as both before and after the exchanges with Alexandria.

Mokattam was my own station. The principle I endeavoured to follow was to multiply star- and telegraph-signal observations at that end of the line where each star-observation must be expected to be the less accurate, and where the rate of the clock was more liable to fluctuation. In some cases an unusually large number of stars were observed, owing to the fact that at each opportunity that occurred it was desirable to secure several good transits, because the skies might cloud over at any time.

Mr. Hunter was at Alexandria. In addition to his work in transmitting cable and land signals, he undertook star-

transit observations on each night, in order to obtain the longitude of Alexandria direct from Greenwich, instead of from Mokattam, by a separate operation.

Mr. Ellis was at Porthcurnow, and, under the Astronomer Royal's direction, arranged the exchanges between Greenwich and that station with Mr. Criswick, who observed stars and signals at Greenwich.

A complete exchange of signals was effected without any hindrance or disappointment on four nights, namely November 14, November 15, November 21, and November 22, 1874. On three nights stars were visible at Greenwich, and on all four nights at Mokattam and Alexandria.

The following was the arrangement of line batteries and condensers, &c., on the cable circuit.

A condenser was employed at the receiving end only between the line and instrument. The mirror was used on the Thomson recorder as arranged by Mr. Bull.

Nearly the same batteries were used throughout at Porthcurnow and Alexandria, namely 40 cells of Mennotti or Daniel on the line. Signals, however, were received at Porthcurnow from Alexandria to test the sensitiveness of the instrument with 12 Mennotti cells; and messages were read from Gibraltar (1161 miles) with 3 Mennotti cells. To keep up the magnetic field, ten and twelve large batteries of the Daniel form were used, the size of the zinc plates being about one foot square, and the resistance of the electromagnet about 16 ohms.

The following are the particulars of the cables employed:—

Porthcurnow to Vigo	620 knots.
Vigo to Lisbon	247 „
Lisbon to Gibraltar	331 „
Gibraltar to Malta	1120 „
Malta to Alexandria	904 „
Total length	3222 „

The electrostatic capacity of the cable is 0·3645 microfarad per knot.

Specific inductive capacity of insulator 0·0729 microfarad.

The total number of cable-signals observed was 956.

By the order of the Astronomer Royal, Mr. Ellis worked out the longitude of Mokattam before the return of the expedition; and the work, as far as the reduction of the exchange of signals was concerned, was then completed. The longitude was slightly modified by the application of corrections for personal equation and also on the final working-up of the whole of the star-observations, which could not be completed by me in camp.

There was no perceptible effect that could be attributed to earth-currents. The mean loss of time before the signal on make of current was visible, was 1.34 sec., and that before break of current signal was 1.28 sec., the mean being 1.31 sec., which is, as noticed, remarkably close to Mr. Varley's estimate of $1\frac{1}{4}$ sec. The final longitude of Mokattam from Greenwich was $2^h 5^m 6^s.32$; the results of the four different nights agreed very closely, the seconds from the observed times and signals uncorrected for personal equation being 6.55, 6.64, 6.61, and 6.55, an accordance which exceeded all expectation. It would appear from this that the longitude was probably correct within two or three hundredths of a second; but it is to be borne in mind that, although there is strong evidence of the personal equations of the observers having remained very constant throughout the nights of the experiment, the amount of each could only be obtained at Greenwich after the return of the expedition, when slight alterations might have taken place in them.

From Mokattam the longitudes of the Suez and Thebes stations were easily obtained; and the work was carried further to the east by Mr. Gill, of Lord Lindsay's party, who, with the Germans, determined the longitude of Suez and Bombay.

XLVII. A Theory of Mr. Crookes's Radiometer.

By Professor CHALLIS, M.A., F.R.S., F.R.A.S.*

I ASSUME that the construction of the radiometer and the phenomena it presents are well known. In the *Philosophical Magazine* for January 1857 I have proposed a "Theory of the Transmutation of Rays of Light," which bears in an essential manner on the explanation of the radiometer I am about to propose; and I have made additional remarks on the same question in the *Philosophical Magazine* for May 1865. According to the views adopted in those communications, rays of light incident on opaque substances are partly reflected without undergoing transmutation and are partly intromitted. The latter part are transmuted into rays of the kind which by their dynamical action produce *heat of temperature* as distinct from *radiant heat*. This is what takes place on the *bright* side of each revolving vane of the radiometer. We may assume that the same quantity of light is incident on the *blackened* side as on the other; but because, as the surface is black, none of the light is regularly reflected, it follows that the whole is transmuted into the heat of temperature. The accession of temperature is therefore greater on the black than on the bright

* Communicated by the Author.

surface. The incident light thus produces an abnormal state of the atoms at and near the superficies of the vane, analogous in some degree to the state of the superficial atoms of a body electrified by friction. Such disturbance of the atoms at the surface must be accompanied by change in the interior, having the effect of causing the atoms to be somewhat more crowded together at one part than at another. (See on these points the article on the Force of Electricity in the Philosophical Magazine for October 1860, and the Theory of Electric Force in 'The Principles of Physics,' pp. 505-555.) With respect to the vanes of the radiometer in their state of continuous perturbation as to temperature, it is clear that the crowding of the atoms will be towards the bright or cooler surface, and that there will be an abnormal increment of density, regular or not, from the blackened to the white surface.

This being admitted, I may proceed to state the theory which I have elsewhere proposed for accounting for the maintenance of the interior gradation of atomic density. I consider that this theory is best given in a "New Discussion of the Hydrodynamical Theory of Magnetism," contained in the Philosophical Magazine for June 1872; but it should be noticed that in a magnet the interior gradation of atomic density results from the peculiar constitution of hardened steel, and not from abnormal superficial conditions. The case of our problem is more analogous to the production of thermo-galvanic currents by Seebeck's experiment. In any case, however, of such gradation the theory supposes that atomic repulsion acting from denser towards rarer parts is equilibrated by molecular attraction acting from rarer towards denser parts. According to the hydrodynamical theory of Attractive and Repulsive Forces (which is given more completely in the Philosophical Magazine for September 1872 than in my previous discussions of it), these forces result from the dynamical action of *ætherial undulations*—those of small breadth and intensity causing repulsion in the direction of their propagation, and those of which the breadth and maximum intensity are large, causing attraction in the direction opposite to that of propagation. Now in all cases of the equilibration of a congeries of atoms by these opposing forces, when terms including the *square* of the velocity of the æther are taken into account, the mathematical reasoning (as given in the Philosophical Magazine for June 1872) shows that the æther itself is accelerated by reason of the contraction of channel due to the constancy of magnitude and form of the atoms, and the increment of their number towards the denser parts. These impressed accelerations maintain a *steady circulating motion* in the midst of the æther, which is always tending, by the inertia of its unlimited mass, to put a stop to such motion. According

to the known law of steady motion, the density and pressure of the fluid are greater as the motion is less. Now in the case of the ætherial current maintained by the variation of atomic density in the radiometer's vane, the direction will be transverse to the parallel surfaces of the vane, and the motion will be least near the black surface because, by reason of excess of temperature, the channel will there be greatest. Hence there will be a decrement of *ætherial density* from the dark towards the bright surface, and the atoms, being immersed in this variation of density, will be urged as if the vane were pushed on the black surface. This agrees with the fact as observed. (It is proved in art. 33 of the communication in the *Philosophical Magazine* for September 1872, that the acceleration of an atom resulting from the variation of density of the æther has a constant ratio to the acceleration of the fluid where the atom is situated.)

Having surmised from the foregoing theory that the rotation of the vanes might be affected by the streams of a magnet in the neighbourhood of the radiometer, I found on trial that this is the case, the rate of revolution being much reduced by the proximity of a magnet of considerable power. This result might be due to the influence of the magnet's streams on those which, according to our theory, cross the vanes transversely, and may be taken to be evidence of the actuality of the latter streams.

The theory shows that the vanes are impelled by a constant accelerative force so long as the same quantity of light falls on them. But from observation it appears that for a certain quantity of incident light they attain a certain rate of revolution and then cease to be accelerated. How is this fact to be explained? It is evident that there must be some amount of *friction* between the glass support and the pivot on which the system of vanes turns; and this, together with the resistance which would be produced by ever so small a residuum of air in the glass globe, may sufficiently account for a limit to the rate of revolution. As respects friction, this explanation appears to be confirmed by a peculiar circumstance which has been very recently noticed—namely, that when the glass globe is caused to float in water and the motion of the vanes is stopped, on throwing light upon them the globe is observed to *rotate*. The reason seems to be that the accelerative force which by the incidence of light is made to act on the vanes, takes effect, by the intervention of friction, so as to move the globe. It is evident that the tendency of the friction is always to make the globe partake of the rotatory motion of the vanes. I give this explanation with reservation, not having yet seen a complete account of this novel experiment.

Cambridge, April 21, 1875.

XLVIII. *On a new Oxide of Sulphur and an analogous Selenium Substitution Product of the same.* By RUDOLPH WEBER*.

IT is known that deeply coloured products are obtained by the action of sulphur, selenium, and tellurium upon sulphuric anhydride or upon fuming oil of vitriol.

In his research on tellurium M. von Reichenstein made the observation that that element dissolves in cold oil of vitriol, and colours the liquid amethyst, and that on the addition of water the tellurium is precipitated in the metallic state. Bucholz obtained a deep-blue-coloured substance by distillation of fuming sulphuric acid with sulphur; at a later time the same compound was obtained by Vogel by the direct action of sulphur upon sulphuric acid containing sulphuric anhydride.

Magnus observed that selenium dissolved in oil of vitriol with the production of an intensely green-coloured liquid, but was again precipitated, for the most part, on the addition of water.

The observations hitherto made do not allow of any conclusions being drawn as to the chemical nature of these coloured substances. Not only does the composition of these bodies remain undecided by the recorded observations, but the very existence of chemical compounds of sulphur, selenium, and tellurium with the constituents of sulphuric acid must yet be regarded as doubtful. The view that the coloured bodies are mere solutions of the elements in acid derives support from the circumstance that the coloured liquids are decomposed by water with deposition of the elementary bodies. In the prosecution of a research upon sulphuric anhydride (the general results of which will be communicated hereafter), I have been led to study the action of sulphur and of selenium upon this compound; and I have obtained bodies of definite chemical composition which dissolve in strong oil of vitriol, as also in sulphuric anhydride (prepared in the ordinary way), to form coloured liquids which agree in all essential particulars with the substances obtained by the above-mentioned chemists. The substances prepared by me are, a new oxide of sulphur, and a selenium substitution product of this oxide.

Before stating the results of my investigation it may be well to give a slight historical sketch of the facts which are known concerning the blue sulphur compound.

Bucholz †, in 1804, obtained a blue distillate by heating a mixture of sulphur and oil of vitriol in a retort. He did not

* From Pogg. *Ann.*, vol. clvi. p. 531. Translated by M. M. Pattison Muir, The Owens College, Manchester.

† Gehlen's *Neues J. der Chemie*, vol. iii. p. 7.

follow up this observation, but contented himself with showing that the liquid fumed strongly in the air, smelt of sulphurous acid, and was decomposed by water with precipitation of sulphur.

F. C. Vogel *, who is usually regarded as the discoverer of this blue-coloured substance, was aware of the observation of Bucholz.

In 1812 this chemist prepared a blue substance by the action of sulphur upon sulphuric anhydride, which he had obtained by heating fuming oil of vitriol ; at the same time he obtained a green and also a brown-coloured liquid, according as he added smaller or larger quantities of sulphur to the anhydride. Vogel looked on these bodies as compounds of the anhydride with varying proportions of sulphur ; the blue compound he supposed to contain the smallest, and the brown the largest quantity of sulphur.

In repeating the experiments of Vogel, Schweigger † observed that the blue compound, obtained by the action of the vapours of heated oil of vitriol upon sulphur, which was contained in a receiver luted to the retort, gradually became green in colour, then brown, and was finally converted into ordinary yellow sulphur.

Berzelius ‡, who formed his opinion as to the nature of this substance after Schweigger's observations were known, considered that the blue liquid contained sulphur dissolved in, but not combined with the acid, and that the substance obtained by the action of sulphuric anhydride upon sulphur was not an oxide of sulphur.

Wach § sought to determine the relative quantities of sulphur and of anhydride in the differently coloured compounds by heating weighed quantities of the constituents in sealed tubes, and observing the colours of the products. The brown compound was produced from a mixture of one part of sulphur with five of anhydride ; the blue, from one part of sulphur with ten of anhydride. Inasmuch as Wach did not separate the substances which he obtained from excess of the materials used, we cannot suppose that he was really dealing with pure compounds ; on distilling the coloured bodies he obtained a brown oily liquid which appeared to be ordinary oil of vitriol. Wach's results therefore give no answer to the question concerning the composition of these substances.

* Schweigger's *J. für Chemie u. Physik*, vol. iv. p. 121.

† *J. für Chem. u. Physik*, vol. xiii. p. 484.

‡ *Lehrbuch*, vol. i. p. 485 (5th edit.).

§ Schweigger's *Journal*, vol. i. p. 1.

W. Stein* agreed with Berzelius in regarding the blue substance produced from sulphur and anhydride as a solution of the former in the latter. The blue colour he believed to be due to the extremely finely divided state in which he supposed the sulphur to be present.

Preparation of the Blue Sulphur Compound.

The isolation of this compound is rendered possible by the fact that it is not soluble in perfectly pure sulphuric anhydride (prepared by careful distillation of the liquid which is obtained by gently warming oil of vitriol), and that any excess of anhydride may therefore be removed. Inasmuch as the compound is stable for some time at a temperature of 10° or 15° , exact quantities of it may be weighed out for analysis. It is only by using pure anhydride that the blue sulphur compound can be obtained in definite shape; ordinary sulphuric anhydride (as will be shown in a subsequent communication) is not pure.

When sulphuric anhydride, prepared without especial precaution, and therefore containing a small quantity of the hydrate, is brought into contact with sulphur, the blue compound is dissolved as quickly as it is formed in the excess of (impure) anhydride, from which it cannot be separated. A portion of the blue substance solidifies along with the anhydride, which is thereby more or less deeply coloured.

If perfectly dry powdered sulphur be added in successive small quantities to pure anhydride, the blue compound separates in the form of small drops, which, being heavier than the anhydride, settle to the bottom of the vessel and there solidify. For the proper carrying out of this operation it is essential that the temperature be about 15° , inasmuch as decomposition of the compound readily takes place at higher temperatures, while at temperatures under 15° the anhydride solidifies. In the solidification of the blue compound heat is evolved sufficient to bring about partial decomposition, unless the temperature be maintained at a sufficiently low point.

In order to prepare pure sulphuric anhydride, the substance obtained by heating fuming oil of vitriol is carefully distilled at as low a temperature as possible; the first portions of the distillate are rejected, and those which succeed are collected in a receiver which fits closely to the neck of the retort, and which is maintained at a temperature of about 15° , care being taken that the distillate does not solidify.

In order to prepare the blue compound, small quantities of

* *J. für prakt. Chem.* vol. vi. p. 172.

most carefully dried flowers of sulphur are successively added to the pure, clear, liquid anhydride : fresh quantities of sulphur must only be added after the first portions have entered into combination with the anhydride, because any excess of sulphur is readily enclosed by the blue compound as it solidifies, and the decomposition of the compound is thereby hastened. Immediately that the sulphur comes into contact with the anhydride it dissolves, with the formation of deep-blue-coloured liquid drops, which settle to the bottom of the vessel. The operation may be conveniently carried out in an ordinary conical glass covered with the lid of a porcelain crucible to prevent the admission of atmospheric moisture. After every addition of sulphur the glass must be agitated, so that the drops which are formed may, by striking against the sides of the vessel, be spread throughout the liquid and allowed to solidify in the form of a crust rather than in little round masses, which are more liable to enclose portions of the anhydride.

The glass should from time to time be immersed in water at a temperature of 12° – 15° , in order to prevent an undue rise of temperature. When about a gram of sulphur has been added, and the sides of the glass are covered with a thin crust of the desired compound, the excess of anhydride (which should remain colourless) is carefully poured off, the last portion being removed by exceedingly careful evaporation, carried out at a temperature not exceeding that of the blood. The solid substance is removed from the glass by means of a rod, placed in a well-covered vessel, and preserved in as cool a place as possible.

Properties of the Blue Compound.

When dry, this compound forms a bluish green easily friable crust, consisting of an aggregate of small papillary crystals which are only visible with the magnifying-lens. The compound is liquid only at the moment of its formation ; it quickly solidifies, and cannot be again fused without undergoing decomposition. This substance decomposes slowly at ordinary temperatures, and more quickly on being heated. During decomposition sulphur dioxide is evolved ; and if moisture be carefully excluded, dry sulphur remains. I have not noticed the evaporation of sulphuric anhydride during the decomposition of this compound. The sulphur which Schweigger noticed remaining in the vessel in his experiments was not to be traced simply to the removal of anhydride which had previously been combined with it, but to the decomposition of the blue compound attended with liberation

of sulphur dioxide. The conclusion arrived at from Schweiger's experiments, viz. that the sulphur is merely dissolved in the anhydride without any true chemical action taking place, is not a legitimate conclusion.

This compound decomposes but slowly in a cool place when protected from the action of moisture; quantities required for analysis may therefore be readily weighed out. The substance readily withdraws water from moist air, and forms a clear brown liquid, which by taking up more water becomes turbid through the deposition of sulphur. On the addition of cold water to this body rapid decomposition ensues with the production of sulphur (which appears partly in the plastic state, partly in the finely divided form in which it is known as milk of sulphur), sulphuric and sulphurous acids, and thio-sulphuric acid or one of the polythionic acids analogous in its reactions to thiosulphuric acid.

On the addition of silver solution to the liquid, a precipitate is obtained which, from being at first yellow, changes to red, then to brown, and finally to black.

Absolute alcohol, and also anhydrous ether, decompose the blue compound with precipitation of sulphur; the behaviour of the compound towards sulphuric acid varies in accordance with the concentration of the acid.

Many of the older observations on this compound are explained by these reactions.

Composition of the Blue Compound.

In order to obtain a quantity of the compound for analysis, the freshly prepared substance was placed in a small thin glass tube provided with a glass stopper and previously weighed. After weighing, the stopper was withdrawn, and the tube was placed in a glass about 80 millims. in height, which was furnished with a properly fitting glass stopper; in this glass a thin layer, about 1 millim. deep, of a mixture of pure and of fuming nitric acid had been previously placed. After inserting the stopper the glass was inclined so that the acid came into contact with the substance, whereupon violent decomposition ensued. The stopper was firmly held in its place until the liquid had entirely absorbed all the red fumes; the absorption was only complete after 12 or 24 hours. The contents of the glass were then diluted with water, and the liquid separated from sulphur by filtration. The sulphuric acid in the filtrate was precipitated as barium sulphate, while the separated sulphur was collected and weighed. The results of analyses carried out in the way described are as follows:—

Substance taken.	Sulphur.	Barium Sulphate.	Total percentage of Sulphur.
0.785	0.151	2.112	56.36
0.851	0.181	2.200	56.94
0.519	0.114	1.315	56.93
1.038	0.235	2.633	57.64
0.910	0.206	2.284	57.26

Mean . . . 57.12

The residue is oxygen.

The composition of this substance is expressed by the empirical formula S_2O_3 , which requires 57.14 per cent. of sulphur. This substance is therefore an oxide of sulphur containing twice as much sulphur as the trioxide. Hitherto I have not been able to form compounds of this substance, nor to adduce facts from which an insight into the constitution of the body might be obtained. This compound has not the character of an acid anhydride, so far as I have yet ascertained; it combines neither with water nor with metallic oxides, but comports itself as an indifferent oxide, resembling some of the oxides of nitrogen.

I propose for this compound the name of *Sulphur sesquioxide*, or *Dithionic oxide*.

An insight into the constitution of this compound may possibly be obtained by studying its behaviour towards dry ammonia; it is, however, difficult to obtain a definite product of this reaction, inasmuch as the compound which appears to be at first formed suffers partial decomposition on account of the heat produced in the action, notwithstanding that the tube be carefully cooled. A peach-red-coloured substance is produced in this reaction; but this body has not yet been obtained altogether free from a yellow and a white substance simultaneously produced. When a mixture of these substances is treated with water, sulphur is separated, and a solution is obtained which contains sulphuric acid and polythionic acids which show the characteristic reactions with silver salts.

Behaviour of the Sesquioxide towards Sulphuric Acid.

The sesquioxide, as has been already mentioned, is insoluble in pure sulphuric anhydride; it is, however, very readily dissolved by a mixture of anhydride and hydrate, even when the latter is present in very small relative quantities. The blue compound cannot be separated from such a solution. Crystals of anhydride separate from the solution in asbestos-like masses, which are more often coloured by the sesquioxide, and which retain their colour for a considerable time if protected from the air and preserved in a cool place. The sesquioxide also dissolves in permanently liquid mixtures of anhydride and hydrate, forming blue solutions which preserve their

colour for weeks when kept in sealed tubes in a cool place. The colour of these solutions, however, gradually changes from blue to green, then to brown, and finally to the appearance of ordinary oil of vitriol. From solutions containing a large quantity of sesquioxide relatively to the amount of anhydride present, sulphur is slowly precipitated as the colour of the liquid changes; if, on the other hand, the proportion of anhydride be large, the solutions remain clear. Sulphurous acid is an invariable product of the decomposition of these solutions. If slightly fuming (or English) sulphuric acid be added in small quantities to the blue liquid just described, the colour changes to green, and, on further addition of acid, to brown, until finally sulphur is deposited and sulphurous acid is evolved. Like changes of colour are brought about by the addition of sulphur in successive small quantities to the blue liquid. By cooling the liquid which has been saturated with sulphur, that element separates out, partly in a crystalline and partly in an amorphous form. I have found that the brown liquids are much less stable than the blue, inasmuch as they lose their colour even after 24 hours. Heat hastens the decomposition of the dissolved sesquioxide; it likewise influences the colour of the solution. A cold solution, which was coloured brown, became green when heated, but returned to its original colour upon cooling. The brown liquid, obtained either by the action of hydrated sulphuric acid upon the sesquioxide, or by acting on a solution of the same body with sulphur, contains a larger amount of sulphur proportionally to anhydride than is found in the sesquioxide. A brown substance is also produced by the action of the vapour of anhydride upon an excess of dry sulphur: this substance in the dry state is very unstable. By allowing vapours of the anhydride to act upon sulphur, the latter is momentarily coloured brown, but the colour quickly disappears.

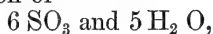
Dry sulphur quickly brings about the decomposition of the dry sesquioxide. I have not been so fortunate as to isolate this horn-coloured substance; and indeed I doubt whether it is a true oxide of sulphur, but rather incline to the view that sulphur behaves towards the blue sesquioxide in a manner similar to that observed in the cases of selenium and tellurium, which, when added to the chlorides of these elements, dissolve therein to a large extent.

I have determined the composition of the mixture of sulphuric anhydride and hydrate, in which the sesquioxide dissolves with the production of a brown liquid, the colour of which, on the smallest addition of anhydride, changes to blue. For this purpose English sulphuric acid was added to a quantity of the anhydride, containing hydrate, until a small

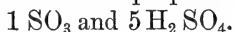
quantity of the blue sesquioxide, which was dissolved in concentrated acid, was taken up by the liquid with the production of a brown colour. This estimation of the sulphuric acid contained in the mixture gave the following results:—

Substance taken.	Barium Sulphate obtained.	Percentage of Sulphuric Acid.
1·356	3·327	84·20
1·400	3·475	84·32

The liquid therefore contained sulphuric anhydride and water in the proportion of



or anhydride and hydrate in the proportion of



Such a solution may be diluted to a certain point without losing the property of dissolving the sesquioxide with the production of a brown liquid; but if this point be overstepped, a decomposition takes place, the liquid becomes turbid and of the colour of ordinary oil of vitriol, while sulphur is deposited. The analysis of such a liquid showed that its composition nearly agreed with that of the hydrate ($\text{H}_2 \text{ SO}_4$).

The blue sesquioxide is therefore dissolved with the formation of a brown liquid in an acid mixture containing an excess of anhydride amounting to one fifth of that contained in the first hydrate.

Acid mixtures which are richer in anhydride dissolve the sesquioxide with formation of blue or green liquids, while acids whose concentration corresponds with that of the first hydrate, or is somewhat less than this, decompose the sesquioxide with the deposition of sulphur.

The analogous Selenium Compound.

Before describing the results of my experiments, I shall give, as in the case of the sulphur compound, a short historical sketch of the work which has been done in connexion with this substance.

Magnus* first observed that selenium was soluble in oil of vitriol. He stated that the solution is coloured green, that the selenium dissolved as such, and that it is only upon warming the liquid, or upon allowing it to remain in moist air, that the selenium undergoes oxidation, while sulphurous and selenious acids are evolved. The statement, that the selenium is dissolved without change, Magnus based upon the observation that addition of water to the green liquid caused the deposition of almost the whole of the selenium, while from the liquid which had been decolorized by boiling, no selenium, or but a very small quantity, was thrown down by a similar treatment.

* Pogg. *Ann.* vol. x. p. 491.

Fischer* differed from Magnus in his interpretation of the formation of the green liquid already mentioned. He supposed that this liquid contained a coloured oxide of selenium, and that the violet liquid obtained by replacing tellurium for selenium, contained a coloured oxide of tellurium dissolved in acid, and that these oxides were decomposed by water with precipitation of the elements selenium and tellurium. He did not think it probable that a mere solution of these elements should be possessed of a colour so different from that of the elements themselves; and he further strengthened his position by pointing out that a considerable part of the dissolved tellurium remained unprecipitated on the addition of water.

Magnus† found that about 2 per cent. of the selenium which had been dissolved was not precipitated by water. The strife as to the nature of this selenium solution has not hitherto been brought to an end, because no direct proof could be brought forward by either side, and also because no coloured oxide of selenium was known. By the action of selenium upon pure sulphuric anhydride, I have succeeded in obtaining a compound analogous to dithionic oxide, in which one atom of sulphur is replaced by one atom of selenium. This substance dissolves in fuming and also in English oil of vitriol, with the production of a deeply green-coloured liquid, which is possessed of the properties of the coloured liquids which have been already mentioned.

Preparation of the Compound.

The behaviour of selenium towards ordinary sulphuric anhydride is similar to that of sulphur: it dissolves easily therein; but the compound which is produced cannot, any more than the dithionic oxide, be separated from this solution. In the pure anhydride (prepared as I have already described) selenium is gradually converted into a dark green viscid tar-like mass, which is denser than the anhydride. By maintaining a temperature of 15° to 17° C., and using a small quantity of selenium, the production of this substance is accompanied by the evolution of but a very small quantity of sulphurous acid, formed by the partial decomposition of the substance. The tarry drops of the selenium compound do not dissolve in an excess of anhydride, which remains uncoloured, and can be separated by decantation. Inasmuch as heat is produced in this reaction, means must be adopted for cooling the anhydride, without, however, causing its solidification. The anhydride used in the preparation of the compound was contained in a glass tube about 6 millims. wide; small pieces of porous selenium were successively dropped into the tube,

* Pogg. *Ann.* vol. xii. p. 153, vol. xvi. p. 118. † *Ibid.* vol. xiv. p. 328.

which was covered with a porcelain crucible-lid. The selenium absorbs the anhydride somewhat more slowly than sulphur does. A fine glass rod is used for determining whether any little particles of selenium remain unacted upon. When the whole of the selenium is combined with the anhydride, the excess of the latter is poured off, the last particles being removed by the application of a gentle heat in the manner already described. Decomposition of the compound may be caused during the heating; but inasmuch as the selenium is more stable than the sulphur compound, this source of error is not to be so greatly feared.

The substance which has been thus prepared forms a solid crust, which strongly adheres to the glass. It is removed by means of a rough glass rod, and quickly brought into a carefully enclosed flask, or better, into a glass tube, which is then sealed.

Properties.

In the moment of its preparation this compound is tar-like and viscid. Upon the removal of excess of anhydride it forms a solid crust, which, under the magnifying-lens, is seen to consist of a mass of minute prismatic crystals. In the compact form this compound is of a dirty green colour; in the state of powder, as it is obtained by rubbing together the crust with a glass rod (of course, in the absence of moisture), its colour is yellow. The compound is more stable than dithionic oxide; it may be preserved in sealed glass tubes, and may even be gently heated without decomposition. Like the sulphur compound, it cannot be fused after the first solidification; at a high temperature it is decomposed, with the evolution of sulphurous and selenious acids, and deposition of selenium. This compound is insoluble in pure sulphuric anhydride; it is, however, soluble in fuming, in English, and in somewhat dilute sulphuric acid. The addition of water causes a violent reaction, resulting in the deposition of selenium in the form of a coherent mass, and in the production of sulphuric, sulphurous, and selenious acids, which remain in solution.

Composition.

The quantities of sulphur and of selenium in the compound were estimated by decomposing with water a weighed quantity contained in a little stoppered flask, and placed within a larger flask, also stoppered. When the action was complete, the precipitated selenium was collected on a weighed filter; and to this was added the small quantity of selenium which was thrown down from the filtrate on the addition of ammonium sulphite. In the liquid thus freed from selenium, sulphurous acid was oxidized to sulphuric by means of a little pure fuming nitric acid, and barium chloride was added so long as a precipitate was obtained. The following numbers were obtained:—

Selenium-Estimation.

Substance taken.	Selenium.	Selenium per cent.
1.428	0.695	48.67
0.930	0.455	48.87
0.682	0.332	48.68
1.855	0.926	48.43
1.230	0.609	49.51
Mean . . .		49.03

Sulphur-Estimation.

Substance taken.	Barium Sulphate.	Sulphur per cent.
1.120	1.581	19.40
0.916	1.327	19.91
0.940	1.333	19.49
0.839	1.225	20.06
Mean . . .		19.71

The composition of this substance may be expressed by the empirical formula SeSO_3 , which requires

Selenium . .	49.68 per cent.
Sulphur . . .	20.12 „

This body is therefore a compound analogous to dithionic oxide, in which one atom of sulphur is replaced by one atom of selenium. These compounds are produced by the immediate addition of one atom of sulphur, or of selenium, to sulphuric anhydride.

The products obtained from each by decomposing them with water show that the collocation of the atoms is the same in each. In both cases the chief product is sulphuric acid, accompanied by sulphurous acid; in the decomposition of the selenium compound selenious acid is also produced; and in the decomposition of the sulphur compound, hyposulphurous acid or one of the polythionic acids is formed. The largest portions of the elements, sulphur or selenium, are separated as such on the addition of water. The constitution of this substance can only be guessed at, inasmuch as it has not been possible to obtain compounds derived from it.

Although ammonia reacts with this compound with the production of a brown-red substance, yet, on account of the energy of the reaction (notwithstanding that the containing vessel was carefully cooled), it has not been possible to obtain this substance in a definite form.

Behaviour towards Sulphuric Acid.

The selenium compound is insoluble in excess of pure sulphuric anhydride; but it is dissolved by anhydride containing hydrate, with the formation of an intensely green liquid, from

which it cannot be again precipitated. If the solvent contains so much anhydride that crystals are formed on cooling, these crystals are more or less coloured by the green selenium compound. The yellow powder dissolves in fuming sulphuric acid, forming a green liquid, which comports itself similarly to a solution of selenium in the same solvent. This liquid, if preserved out of contact with moisture, retains its colour; it is decomposed by water with deposition of the greater part of the dissolved selenium, 94 to 96 parts of selenium being precipitated for every 100 parts dissolved.

The solutions are decolorized by treatment with solution of sulphurous acid, with oxidation of selenium to selenious acid.

That the colour of green solutions of selenium in sulphuric acid is due to the formation of the compound SeSO_3 , and subsequent solution of the same, is shown by the fact that selenium is not dissolved by cold English sulphuric acid, while the compound SeSO_3 is dissolved by this acid with the formation of a green liquid. While dithionic oxide is decomposed by the first hydrate of sulphuric acid with deposition of sulphur, the analogous selenium compound is dissolved by this hydrate, and even by an acid somewhat more dilute than this; there is a point of dilution, however, at which decomposition takes place. By careful addition of dilute acid to the green solution, it is possible to obtain a brown liquid corresponding with that obtained from a solution of the sulphur compound. This brown liquid is decomposed by further dilution, by warming, or spontaneously after a lapse of some time; the decomposition is accompanied by the deposition of red selenium. The selenium compound dissolves, with the production of a brown-coloured liquid, in sulphuric acid of 1.806 specific gravity, containing about 88 per cent. of the first hydrate. In acid of 1.761 specific gravity, containing about 83 per cent. of the same hydrate, the compound also dissolves; but the brown liquid so produced soon suffers decomposition, with deposition of selenium.

The author is engaged with an investigation of the compounds produced by the action of tellurium and of iodine upon sulphuric anhydride.

XLIX. *Notices respecting New Books.*

A Manual of Qualitative Chemical Analysis. By WILLIAM DITTMAR, Professor of Chemistry in Anderson's University, Glasgow. Edinburgh: Edmonston & Douglas. 1876.

IT was said of Whewell that he had one failing—omniscience. In so far as the subject matter of this book is concerned, the author appears to suffer under a similar calamity.

The book contains too much information—too much, that is, for

the purposes of the ordinary chemical student. In the introduction it is set forth that "this book is intended for the use of students who, after they have mastered the first rudiments of chemistry, enter a laboratory to work *under a teacher*, while at the same time they *continue their study of theoretical chemistry*." (The italics are the author's.) The student who, fresh from his mastery of the first rudiments of chemistry, enters a laboratory with this book as his guide, even if its teaching be supplemented by that of a chemical tutor, would, we should imagine, before long devoutly wish that he had never set foot in such a labyrinth as that of qualitative analysis, to lead him out of or through which there appears to be no clue.

Such a multitude of facts concerning the reactions of metals and of acids are in this book presented to the bewildered gaze of the student, in the separation of each group so many courses are open to him, that he must be either bold almost to rashness, or possessed already of a large stock of chemical knowledge, who would venture to adopt one method in preference to another, or (to use the language of the author) from those which are "potentially" applicable to choose that which is "substantially" the best. The style in which the work is written is peculiar. Simplicity would appear to be in the author's eyes the unpardonable sin. Such expressions as "the solution potentially contains such and such metals, but substantially consists of," the "complex of metals," "in the heat" as opposed to "in the cold," &c., grate upon the ear of the English reader.

As an example of that want of straightforward simplicity for which we blame the author, we might refer to par. 464, p. 277, in which the reactions of prussic acid are detailed. This acid is usually called hydrocyanic, and its formula is written HCN : in one part of the paragraph in question the usual name is employed, in another the name *cyanhydric*. Sometimes the formula is written NCH , sometimes HNC . Of course these are but small things; nevertheless to the ordinary student these little changes of name and formulæ, when made for no apparent reason, are perplexing. and only perplexing.

What does the author mean by calling glass an *alloy* of alkaline silicates with silicates of other metals? Such a use of the name alloy is not sanctioned, so far as we are aware, by any of our standard writers on Chemistry. Again, the phrase "hydrogen salt of an acid" is used as synonymous with the acid itself. Of course the term salt may be used to include acids; but if we speak of the salt of an acid the phrase seems to apply to a derivative of that acid; if, however, it means the acid itself, then we are calling a substance and a derivative of that substance by the same name. We mean to indicate one thing, but are implying the existence of two. The application of such a name as "*Native nitrogenized carbides*" to the organic alkaloids, will, we think, amaze many chemists.

When the student is told that he must test the reagents which are to be used in any given process, he is directed to make an

experiment *au blanc*; when he is to prepare a special reagent for a special test, he is told to prepare that reagent *ex tempore*. But perhaps it is scarcely fair to condemn the style of a book which is not written in the native tongue of the author. The well-known skill of Mr. Dittmar as an analyst is a guarantee of the general correctness of the methods given in the book. In the statement of so many reactions it is almost impossible that no mistake should occur. On page 229 the action of nitric upon hydrochloric acid is said to produce the substance NOCl_2 ; Tilden has, however, shown that no such substance exists, and that the reaction in question results in the formation of NOCl .

Hitherto we have been looking at the work before us as a textbook for the ordinary chemical student. If, however, we view it as a book of reference for the very advanced student, or for the chemical teacher, it must appear worthy of the highest praise. In no other English manual is there gathered together such stores of information on all matters connected directly or indirectly with chemical analysis; nowhere are the difficulties which beset the complete analysis of a complex substance so fully pointed out and so carefully guarded against. In no book, we may add, is there presented so complete a scheme of analysis for detecting the non-metallic radicals. The reader is directed in the right way; but it is left to himself whether he shall walk in it or not. This want of authoritative instruction, while rendering the book unsuited to the ordinary student, makes it admirably adapted for bringing out any latent power of scientific reasoning which may be possessed by the more advanced analyst.

The author has not contented himself with detailing the ordinary methods of analysis; he has brought together a host of facts about the general reactions of classes of chemical substances, metallic and non-metallic; and from these reactions he has sketched out methods of analysis which are theoretically, and, we should suppose, in general practically available. From his own view-point, which includes the whole range of analytical reactions, it cannot be but that the author should see difficulties in the application of almost every method. These difficulties he has nowhere attempted to hide. Where he can suggest a method for overcoming them he makes such a suggestion; otherwise he leaves them to be overcome, if possible, by the student himself. To the truly scientific analyst who has already an extensive knowledge of chemical reactions and general principles, this book will be simply invaluable; but to the student it will be distracting.

L. Intelligence and Miscellaneous Articles.

THE INFLUENCE OF TEMPERATURE ON MAGNETIZATION.

BY J.-M. GAUGAIN.

I HAD the honour of presenting to the Academy the results of some previous researches on this subject, in a Note inserted in the *Comptes Rendus*, Feb. 1, 1875, which I will briefly recapitulate:—

1. If a steel bar be put in contact with the pole of a magnet, and, for a point M of the bar, the value of the current of demagnetization at the ordinary temperature be determined, and the bar be afterwards slightly heated with a spirit-lamp, the value of the demagnetizing current corresponding to the point M is found to be very notably augmented.

2. When the bar is allowed to cool in contact with the magnet, the magnetization does not retrograde, but receives a slight increment.

3. When the bar has returned to the ordinary temperature, removing it from the magnet for a few moments is sufficient to cause a portion of the increment resulting from the heating to disappear : when contact between the magnet and bar is reestablished, we do not find the same total magnetism as before contact was broken.

The new facts which I have ascertained are as follows :—

4. When the steel bar put in contact with the magnet, instead of being very moderately heated, has its temperature gradually raised until it assumes the blue tint, the magnetism at first rises, reaches a maximum, and then retrogrades. In a series of experiments upon a bar of 24 centims. length and 10 millims. diameter I ascertained that, when one end of the bar was put in contact with the pole of a magnet, the total magnetism of the middle point was represented, at the surrounding temperature, by the number 41·6 ; on gradually heating the bar, I found that the magnetism took in succession the values 44, 51, 55·2, 52, 52·8, 52·1, 50, 48·5, 48, 48.

5. When, after being strongly heated, the bar is left in contact with the magnet during the whole time of cooling, the total magnetism is augmented in proportion as the bar cools ; and on its arrival at the surrounding temperature, the magnetism retains a value much superior to that which it had before the heating. In the series of experiments just now mentioned the value of the total magnetization was 63·2 after the heating, while before it was only 41·6.

6. We have seen above (4) that when the temperature of the bar exceeds a certain limit, its total magnetism diminishes, instead of continuing to increase. One might infer from this that the magnetization of the bar brought back to the ordinary temperature would likewise be less, if the temperature to which it was raised exceeded the limit of which I have just spoken. I have proved that it is not so. The total magnetization of the bar, restored to the ordinary temperature, is the greater the more the bar has been heated, at least provided its highest temperature was below that which gives to steel the blue tint.

7. When the bar has returned to the ordinary temperature, it suffices, as I have said (3), to suppress contact between the magnet and the bar for a few seconds in order to cause the loss of a part of the increment of magnetism resulting from the heating—but only a part ; even after breaking contact the total magnetization remains greater than before the heating ; and I have verified, in a great number of experiments, that the increase of magnetism is, at least approximately, equal to the increment of the permanent magnetism.

In all these last experiments the magnetization was effected by putting one extremity of the bar in contact with the pole of a magnet ; but I have ascertained that the results are absolutely the same when one extremity of the bar is introduced into an induction-coil traversed by a current.

These new facts cannot be explained in so simple a manner as those which I previously made known (nos. 1, 2, 3). These latter can be accounted for, as I have intimated, by considering only the variations of the coercive force which result from the variations of the temperature. When this is raised, the coercive force is lessened and the magnetism will be augmented, supposing the magnetizing force and the antagonistic molecular force to preserve sensibly the same values between the limits of temperature considered. When the bar is cooled the coercive force increases ; but as it is a passive force, it cannot impress upon the molecules a movement inverse to that which they executed during the heating. The acquired magnetism should therefore persist ; and we have seen (no. 2) that in fact there is no retrogression during the cooling. This non-retrogression appears to me to characterize the coercive force ; for if that force did not exist and the molecules were solicited exclusively by the active forces, the magnetism would be the same during the heating and during the cooling when the bar passed through the same temperatures.

To account for the facts nos. 4, 5, and 6, it appears to me indispensable to have recourse to other considerations. No. 4 would be explained by admitting that the magnetizing force diminishes when the temperature rises ; and this is a point generally admitted. But to account for nos. 5 and 6 it seems necessary to introduce a new hypothesis, which consists in supposing that, when the superficial layers of a steel bar are heated sufficiently to be incapable of receiving any further notable magnetization, they permit the deeper layers (which were not magnetized at all at the ordinary temperature) to receive a certain degree of magnetization. This hypothesis admitted, facts 5 and 6 are explained : the total magnetization of the bar, after complete cooling, is the greater the more the bar has been heated, because the number of layers which partake of the magnetization increases with the temperature ; the superficial layers, according to the hypothesis, are not sensibly magnetized as long as the bar is strongly heated ; but they receive magnetism when the bar is sufficiently cooled ; and this magnetism is added to that of the deeper layers.

If one admits that the magnetizing force exerted by the current of an induction-coil upon a steel bar diminishes when the temperature of the bar rises above a certain limit, one can hardly refuse to admit that reciprocally the inducing action exerted by the bar upon the closed circuit of a coil surrounding it must equally diminish when the temperature increases beyond the same limit. Therefore, even if in this case the orientation of the molecules should be invariable, the demagnetizing current must diminish ; and as this current serves me for measuring the magnetization, the magnetization must itself undergo an apparent diminution. This variation

of the inductive force must contribute a certain share to the decrease of magnetism ascertained during the heating (4), and to the increase ascertained during the cooling (5); and the same variation may also explain other facts, which I propose to discuss in a sequel to the present memoir.—*Comptes Rendus de l'Académie des Sciences*, vol. lxxxii. pp. 685–688.

PRELIMINARY NOTE ON PHOTOGRAPHING THE LEAST-REFRACTED PORTION OF THE SOLAR SPECTRUM. BY CAPT. W. DE W. ABNEY, R.E.

Within the last two years there have been many attempts made to photograph the least-refrangible rays of the spectrum; and though stated to have been accomplished by the elder Draper on a daguerreotype plate in the early days of photography, yet there is reason to believe that this was effected by the reversing action of these rays on slightly exposed sensitive iodide of silver, and that really no direct photographic impression had been obtained till Dr. Vogel published an account of a method for effecting this object in 1874. More recently, Capt. Waterhouse, of the Bengal Staff Corps, has followed up Dr. Vogel's researches, and with a larger share of success than the originator. Both these gentlemen have had resort to aniline dyes of varying colour, using them with the preservative to the plate; and they state that the red end of the spectrum is most sensitive when a red dye (naphthaline) or rosine is employed, and propound generally that the rays that can be photographed are dependent on the colour of the dyes employed: that colour, and not the ingredient added to the sensitive silver salt, is the essential.

Lately I have had occasion to investigate the subject, and have met with such good results that I desire to communicate a preliminary Note to the Society. I have carefully repeated the previous experiments of Vogel and Waterhouse, and only attained partial success with them, though there was sufficient evidence to show that with the dyes the desired end would be attained. Looking more carefully into the chemistry of the question, however, it struck me that the same results might be obtained by other means than colour; and experiments were undertaken with a large series of gum-resins and other oxidized hydrocarbons (added to ordinary collodion), some colourless and some not. With every gum-resin was obtained a considerable extension of the photographic spectrum below the lines *b*; and at length I procured a resin forming with silver a perfectly white compound that seems to be capable of taking an image far below the line *A*. At present, I find that the longer the exposure given, the greater the length of the spectrum impressed; and I believe that, with patience, a very large part of the absorption-lines lying beyond that point may be registered, as I have obtained impressions which are constant in each plate exposed. The past winter has been of a most unsatisfactory nature for this kind of work, and it is only at long intervals that I have been enabled to catch the sun for this purpose. I am earnestly

looking forward to brighter sunshine, when I have not the least doubt that I shall obtain sharp fiduciary lines in the ultra-red. The addition of any colour to the preparations has been any thing but beneficial, retarding the action greatly; and, from what is apparent, the virtue of the dyes used consists simply in the fact that they are hydrocarbons of some kind, probably combined with the faint traces of silver always left in the film of the sensitive plate.

Experiments with potassium bichromate have also been undertaken; and the result shows that (where partially insolated) the rays which usually have no effect on the sensitive compound are capable of continuing the action set up, as I pointed out in the year 1872. Iron and uranium have also yielded prolonged spectra.

The first experiments were carried out with a single prism of 60° , and a lens to the camera of 4-feet focus; the later experiments have been made with a direct-vision spectroscope of 9 prisms, being equivalent to about three single prisms, and with 4 prisms in battery. The same camera and object-glass have been employed.—*Monthly Notices of the Royal Astronomical Society*, March 1876.

ON THE OCCURRENCE OF PHOSPHATES IN THE CAMBRIAN ROCKS.

BY HENRY HICKS, ESQ., F.G.S.

In this paper the author showed from experiments that the Cambrian strata in Wales contain a far greater amount of phosphate and carbonate of lime than had hitherto been supposed. The results published by Dr. Daubeny some years ago, and which have since received the support of some eminent geologists, were therefore proved to be entirely fallacious when taken to represent the whole Cambrian series; for though some portions show only a trace of these ingredients, there are other beds both interstratified with and underlying these series, which contain them in unusually large proportions. The author therefore objects to look upon Dr. Daubeny's experiments as tending in any way to prove that the seas in which these deposits had accumulated contained but little animal life, and that we here approach the borders of the lower limit of organic existence. He contended that the presence of so much phosphate of lime, and also of carbonate of lime, as was now proved by analyses made by Mr. Hudleston, F.C.S., Mr. Hughes, F.C.S., and himself to be present in series of considerable thickness in the Longmynd group, Menevian group, and Tremadoc group, showed that animal life did exist in abundance in these early seas, and that even here it must be considered that we were far from the beginning of organic existence. The amount of phosphate of lime in some of the beds was in the proportion of nearly 10 per cent., and of carbonate of lime over 40 per cent. The proportion of phosphate of lime, therefore, is greater than is found in most of what have been considered the richest of recent formations. The amount of P_2O_5 was also found to increase in proportion to the richness of the deposit in organic remains. It was

found that all animal and vegetable life had contained it from the very earliest time; but it was apparent that the Crustacea were the chief producers of it in the early seas, and of the Crustacea the trilobites more particularly. It was always found where they were present, and the shells of some of the larger trilobites, as now preserved, contained as much as from 40 to 50 per cent. of phosphate of lime. The analyses made by Mr. Hudleston and the author of recent Crustacea proved that they also contain P_2O_5 in very considerable proportions.

In the second part of the paper the author showed that where intrusive dykes had passed through or between the beds containing the phosphate of lime, the beds for some distance on each side of the dykes had undergone a considerable change. Scarcely a trace of the P_2O_5 or of the lime was now to be found in them, though it was evident that, before the intrusions into them had taken place, they, like the other portions of the beds, had contained both ingredients in considerable proportions. It was well known that heat alone could not separate P_2O_5 from lime; therefore he found it difficult to account for this change in the character of the beds, unless it could be produced by gases or watery vapour passing into them at the time the intrusions took place. He thought it even probable that the dykes, which in some parts are found to contain a considerable amount of lime and also of P_2O_5 , might have derived these, or at least some portions of these, from the beds through which they had been forced, and which must have been broken up and melted as they passed through them. There are no contemporaneous tuffs known in Wales of earlier date than the Llandeilo beds; and he thought these dykes belonged to that period, and that they were injected into the Lower Cambrian beds after from 8000 to 10,000 feet of deposit had been superimposed. In an agricultural point of view the author considered that the presence of so much phosphate of lime in some of the series of beds must be a matter of great importance; and on examining the districts where these series occurred, he invariably found the land exceedingly rich.

Mr. Hudleston gave the results of the analyses made by him at the request of Mr. Hicks. He found in a portion of dark grey flaggy rock taken from close to a fossil 1.62, in a portion of black slaty rock containing trilobites but in contact with trap 0.11, in a portion of the shell of a trilobite 17.05, and in the trap above mentioned 0.323 per cent. of phosphoric anhydride. A lobster-shell dried at 100° C. gave 3.26, an entire boiled lobster (undried) 0.76, and a boiled lobster without shell 0.332 per cent. of P_2O_5 . If the analysis of an entire lobster was correct, he estimated that a ton of boiled lobsters would contain about 17 lbs. of phosphoric anhydride. In the analysis of the shell of a Trilobite there appears to be a great excess of phosphoric acid, which Mr. Hudleston thought must be due to substitution.—*Abstracts of the Proceedings of the Geological Society*, No. 299.

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LI. *On the Simultaneous Sounding of two Notes.*

By Dr. RUDOLPH KÖNIG, *Paris**.

IF two notes are produced upon the same instrument, or by the vibrations of two bodies which are closely connected together through a third, there ensue some very intricate phenomena, which are partly produced by the reaction of the two sources of sound upon each other, and the action of both upon the connecting body, and partly also have their origin in the continuance of the two sound-waves in the air. It is my present intention in the following pages to submit to a closer examination only those phenomena which arise from the coexistence of two sound-waves in the air; and I have therefore used for the demonstration of these waves only such sources of sound as were absolutely isolated from each other, and could not possibly act upon each other directly, or combined together upon a third body. As, further, the waves produced by clangs† must always be considered as a combination of waves of simple notes, and as therefore it may remain doubtful when clangs are employed whether the phenomena observed are produced by the fundamental notes or by the over-notes, I have been careful in these experiments so to select the sources of sound that they should only produce the simplest possible notes. For the low notes I used very stout tuning-forks, mounted on isolated iron frames, and placed in front of large sounding-boxes; for the upper notes

* Translated from Poggendorff's *Annalen*, vol. clvii. p. 177. And communicated by W. Spottiswoode, M.A., LL.D., V.P.R.S.

† By "clang" is meant the entire sound emitted by an instrument when sounding a musical note.

simply powerful tuning-forks, whose intensity of tone required no further reinforcement.

The whole series of tuning-forks and sounding-boxes which I made use of for these experiments was as follows:—

1. Five tuning-forks which without weights gave the notes double G, C, E, G, c (sol_{-1} , do_1 , mi_1 , sol_1 , do_2). Each of the four higher forks can, by means of its sliding weights, be tuned down to the note of the next deeper fork. The double-G fork can be lowered by a couple of sliding weights to double E, and by another couple as far as to double C ($\text{do}_{-1} = 64$ v. s.); and this latter limit may still be exceeded by increasing the weights upon the slides. The positions of the sliding weights upon these forks are marked at intervals of a single vibration for the octave from double C to C, and by a double vibration for the next higher octave.

The prongs of the lowest fork are 35 millims. in thickness, 55 millims. in breadth, and about 75 centims. in length. The prongs of the other four forks are 39 millims. thick, 55 millims. broad, and their length varies from about 70 centims. to 49 centims. These five forks, without their stands and sliding weights, weigh 130 kilogrammes.

2. Eight tuning-forks, which without weights are tuned to the notes $c, e, g, c', c', e', g', c''$ ($\text{do}_2, \text{mi}_2, \text{sol}_2, \text{do}_3, \text{do}_3, \text{mi}_3, \text{sol}_3, \text{do}_4$), and by means of their sliding weights are capable of producing all the intermediate notes also. Their prongs are 26 millims. thick, 26 millims. broad, and from about 59 to 19 centims. long.

The tuning-forks for the octave from c to c' have a division for placing the sliding weights at from two to two double vibrations, and for the next higher octave from four to four.

3. Nine forks tuned to the scale of c'' to c''' , and to the seventh harmonic of the small c , of which the prongs are 25 millims. broad, 25 millims. thick below, and diminish to about 12 millims. less at the ends. Their length varies from about 20 to 13 centims.

4. Twelve tuning-forks for the notes of the scale from c''' to c^{iv} , the eleventh, thirteenth, and fourteenth harmonics of c , and the note of 2389.3 v. s., which forms with c' (512 v. s.) the ratio of 3 : 14, with prongs 15 millims. in breadth, 10 millims. in thickness below, and about 7 millims. at the ends, and from about 9 centims. to 6 centims. in length.

5. Eleven forks for the notes of the scale from c^{iv} to c^{v} , and the eleventh, thirteenth, and fourteenth harmonics of c' . The breadth of the prongs is 23 millims., the thickness below 18 millims., and at the ends about 9 millims. Their length varies from about 8 to 5 centims.

6. A series of eleven tuning-forks for notes between b''' and c^{iv} , and a series of nine tuning-forks for notes between 7936 v. s. and c^v (8192 v. s.), with prongs 14 millims. in breadth, and below about 8 millims. in thickness.

7. Three pairs of resonators to strengthen the notes from C to c'' , provided with clamping-screws, so that they may with the greater precision be tuned to each note which requires reinforcement. They are made of brass, and mounted on iron frames. At the opening of each of these resonators two side plates can be applied, in case the tuning-forks cannot, in consequence of their weights, be brought near enough to the opening, and it is desirable to lose as little as possible of their action on the body of air. Besides this, each clamp, close to the screw which goes through and moves it, is bored through, and provided with a small pipe, which is generally closed, but which can be opened, in order that by its means, with the help of an india-rubber tube, the ear may be put in direct communication with the body of air within the resonator.

The two resonators which reinforce the notes from C to g are 30 centims. in diameter, 1 metre 15 centims. in length, and the opening in front is 27 centims. long and 12 centims. broad. The two resonators which can be tuned from C to g' are 25 centims. in diameter, 50 centims. in length, and their opening is 23 centims. in length and 7 centims. in breadth. The third pair of resonators reinforce the notes from g to c'' . Their length is 36 centims., their diameter 25 centims., and their opening is 15 centims. in length and 7 centims. in breadth.

I. *Primary Beats and Beat-Notes.*

A. *Intervals with the fundamental note C (=128 v. s.).*

If at the same time with the deep, simple, and strong note C (128 v. s.), produced by means of a large tuning-fork placed in front of a resonator, a second note is produced in the same manner, which, starting from unison, is gradually raised higher and higher, the beats which ensue after the disturbance of the unison become gradually quicker. When the higher note has reached to 152 or 156 v. s. (that is, between D and E), the beats, which till then were heard separately to the number of twelve to fourteen, change to a roll, which increases in rapidity till the interval of the fourth is nearly reached, about 171 v. s. (twenty-two beats), without losing its simple character. When the fourth is passed, there occurs a confused but always very loud rattle, which lasts till above the fifth, until when close upon the sixth, at about 212 to 216 v. s., it begins to lose its

confused character, and changes into a still rapid but simple roll, which becomes so much slower between the sixth and the seventh that at 233 and 236 v. s. twelve and ten single beats can already be counted, which at the seventh, $B=240$ v. s., decrease to eight, at 244 v. s. to six, and become gradually fewer in number, till at the octave of $c=256$ v. s. they at last cease altogether.

As the number of vibrations of the primary notes can be directly read off from the tuning-forks, it will be found that the number of single distinguishable beats near the unison is equal to the difference of the double vibrations of the two primary notes, and that of the beats near the octave is equal to the difference of the double vibrations of the higher of the two primary and of the octave of the lower note.

The above result can be shortly expressed in the following manner. Each interval $n : n'$ (less than the octave) exhibits two sorts of beats, whose number is equal to the positive and negative remainder of the division $\frac{n'}{n}$, that is to say, equal to the two numbers m and $m' = n - m$, which we obtain by stating $n' = n + m = 2n - m'$. I shall in future, for the sake of brevity, call the beats m lower beats, and the beats m' upper beats. If we increase the interval between two notes from the unison to the octave, the number of lower beats increases from 0 to n , and that of the upper beats diminishes from n to 0. At the fifth the number of both kinds of beats is $= \frac{n}{2}$. If m is much less than $\frac{n}{2}$, only the lower beats are audible; if m is much greater than $\frac{n}{2}$, we hear only the upper beats; and if m is nearly equal to $\frac{n}{2}$, both kinds of beats, m and $n - m$, may be distinguished at the same time.

The lower beats are more powerful than the upper beats; and their audibleness extends consequently further beyond the fifth than that of the upper beats below the same note.

In the octave from C to c , which we have now been considering, it is very difficult to distinguish, through the loud and confused rattle of the upper and lower beats above and below the fifth, the rhythm which belongs to both these kinds of beats, as the number both of lower and upper beats is always so great that heard alone they would produce a very rapid roll. I only succeeded, therefore, in bringing to special and absolutely clear proof both kinds of beats during their

existence by choosing as the fundamental note of the interval a much deeper note than C, namely double E (80 v. s.).

The great fork had on one of its prongs a wooden board 24 centims. broad and 40 centims. long, and, by means of a powerful electromagnet placed between its prongs, was made to vibrate strongly in a space of from 12 to 15 millims. I held my ear to this board, while I brought more or less close to it a tuning-fork with sliding weights and divisions, which I held loose in my hand. If we experiment in this manner, and raise the note on the latter tuning-fork from 80 v. s. gradually higher and higher, the first single audible beats are again lost in a roll and rattle which continue beyond the fifth (20 beats). At 144 v. s., when 32 lower beats and 8 upper beats are produced, the latter begin to be distinguishable. At 148 v. s. ($m=34$, $m'=6$) and at 150 v. s. ($m=35$, $m'=5$), beside the rattle of the 34 or 35 lower beats, the 6 and 5 upper beats can be clearly heard. A good idea may be obtained of the sound produced if we curve the tongue as in forming the letter R, while we force the air out of the mouth in short strong puffs, instead of in one continuous stream.

While on the subject of this experiment with the deep double-E fork, I may remark by the way that it is extremely difficult to produce very deep simple notes of any intensity. As I was anxious to make my experiments upon beats by means of notes which should have the widest possible intervals with the smallest possible difference in the absolute number of vibrations, I constructed for the notes of the double octave (64—188 v. s.) two large wooden resonators, one 40 and the other 60 centims. high and wide, and both 2 metres long. Like the before-mentioned brass resonators, they were fitted with clamping-screws, so that they could be tuned with the greatest precision, and the openings could be increased or diminished at pleasure; but the effect which I obtained from them in connexion with the powerful forks was so small that I lost more in intensity by taking one of these deep notes for the fundamental note than I gained by the lesser number of vibrations.

If we increase the interval of the octave 128 : 256 v. s., at which we have now arrived by retaining the fundamental note C, while we again raise the second note proceeding from the octave higher and higher, we produce again the single audible beats, which, when they have attained the number of 10 to 12 at 276 to 280 v. s., change into a simple roll, which at 296 v. s. (20 beats) is transformed into a confused rattle. This rattle soon becomes weaker; and between e and f , about 332 to 336 v. s., the clang of the two notes only allows a mere roughness

to be perceived, out of which, however, again at 344 v. s., a clear quick roll appears, which gradually becomes slower, till at 360 to 364 v. s. 12 to 10 beats become singly audible; these at 368, 372, 376, and 380 v. s. decrease to 8, 6, 4, and 2, and at $g=384$ v. s. (1 : 3) disappear.

The number of the beats audible close to the octave is equal to the difference of the double vibrations of the higher notes and of the octave of the fundamental note; and the number of beats close to the twelfth is equal to the difference of the double vibrations of the higher notes and the twelfth from the fundamental note.

The order of the intervals here observed of this second period, from $n : 2n$ to $n : 3n$, is therefore precisely the same as that which we observed of the intervals of the first period from $n : n$ to $n : 2n$. Each interval, $n : 2n + m$ or $3n - m'$, again exhibits two kinds of beats, which are equal to m and

m' : if m is much less than $\frac{n}{2}$, we hear only the lower beats;

if m is much greater than $\frac{n}{2}$, only the upper beats can be distinguished; and if m is about the same as $\frac{n}{2}$, the two kinds of

beats exist together. In this period $m = \frac{n}{2}$ in the interval $2 : 5$ ($e=320$ v. s.).

The beats in the interval $n : 2n + m$ are therefore equal to those in the interval $n : n + m$.

In this period also the upper beats are weaker than the lower; and both upper and lower beats are weaker than the corresponding beats in the first period.

The next higher period reaches from $C : g$ to $C : c'$, $n : 3n$ to $n : 4n$; and its centre, in which $m = \frac{n}{2}$ is in the ratio of 2 : 7 (128 : 448 v. s.).

We find in this the same order as in the two first periods, only we cannot follow the two kinds of beats quite so far, as they have again become weaker than in the former periods. If, beginning at g (384 v. s.), we again raise the second note higher and higher, the first single audible beats fall into a roll at 404 v. s. (10 beats), which at 420 v. s. becomes a weak confused rattle. This changes at about 456 v. s. to a mere roughness, from which another clear rattle is only distinguished at 480 to 484 v. s. (16 to 14 beats), which becomes slower till at 492 v. s. we have 10 single audible beats, gradually diminishing in number till at c' (512 v. s.), the double octave, they entirely disappear.

The beats of an interval $n : 3n+m$ or $4n-m'$ are again equal to m and m' .

In the period of $C : c'$ to $C : e'$, of $n : 4n$ to $n : 5n$, the beats can be followed to only a less distance. The lower beats at the number of 8 to 10 change into a roll; but this at 552 v.s. (20 beats) is already so weak that it is hardly more than a mere roughness. At 560 (24 beats) even this is no longer distinguishable, and the two notes from this point form a pure clang. Only at 616 v.s. does the roll of 12 beats again appear from the pure clang, which then passes into the single audible beats, which disappear at 1 : 5 (128 : 640 v.s.).

In the period $C : e'$ to $C : g'$, $n : 5n$ to $n : 6n$, the lower beats are only clear to about 10, and disappear at 664 v.s. (12 beats). The upper beats are feebly audible at 748 v.s., and only at 752 v.s. (8 beats) become singly quite clear.

In the period $C : g'$ to $C : 896$ v.s., of $n : 6n$ to $n : 7n$, the lower beats are only quite distinct up to 780 v.s. (6 beats), and disappear at 784 v.s. The upper beats are feebly audible to the number of 6 at 884 v.s., and only become quite distinct at 888 v.s. to the number of 4.

In the period of $C : 896$ v.s. to $C : e''$, $n : 7n$ to $n : 8n$, the lower beats can be heard clearly to the number of 4 at 904 v.s. They disappear at 908 v.s., 6 in number. Four upper beats are perceptible at 1004 v.s. The two beats at 1008 v.s. are quite distinct.

I succeeded occasionally in perceiving a few beats in the ratio of $C : d''$ and even of $C : e''$ (1 : 9 and 1 : 10); but these were very weak, and could not have been perceived at all by any ordinarily correct ear not specially trained for the purpose, as all those above described can be.

It has always hitherto been assumed that beats can only be directly produced from two notes which are close to the unison, and that the beats of all wider intervals must be produced with the aid of resultant notes. According to this, in the interval $C : c'' - 2$ v.d., which as we have seen allows two beats to be distinctly heard, these beats must have been produced in the following manner :—

$c'' - 2$ v.d. with C ($8n - 2$ with n)	must have produced	892 v.s. ($7n - 2$)
892 v.s. „ C ($7n - 2$ „ n)	„ „	$g' - 2$ v.d. ($6n - 2$)
$g' - 2$ v.d. „ C ($6n - 2$ „ n)	„ „	$e' - 2$ v.d. ($5n - 2$)
$e' - 2$ v.d. „ C ($5n - 2$ „ n)	„ „	$c' - 2$ v.d. ($4n - 2$)
$c' - 2$ v.d. „ C ($4n - 2$ „ n)	„ „	$g - 2$ v.d. ($3n - 2$)
$g - 2$ v.d. „ C ($3n - 2$ „ n)	„ „	$c - 2$ v.d. ($2n - 2$)
$c - 2$ v.d. „ C ($2n - 2$ „ n)	„ „	$C - 2$ v.d. ($n - 2$)
$C - 2$ v.d. „ C ($n - 2$ „ n)	„ „	two beats

Of all these intermediate notes I have been able to discover no trace; and the note $c'' - 2$ v. d. (1020 v. s.) has besides comparatively so little intensity, even when its beats are most distinctly audible with C, that it seems absolutely impossible that it should produce any (even the very smallest) practical combination-note in connexion with other notes; and it would be still more incredible that it should be the origin of a whole series of combination-notes. It is therefore far more natural to derive the beats of the harmonic interval, as well as those of the unison, directly from the formation of the sound-waves, and to consider that they arise from the periodically interchanging coincidences of the similar maxima of the notes n and n' , and of the maxima which have opposite signs.

In the beats of these harmonic intervals, as well as in those of the unison, the similar maxima will either come exactly together, or else with two successive vibrations of the fundamental note; maxima of compression of the higher notes will slightly precede the maximum of compression of the first vibration and follow the second, so that the centre of beating will lie between these two; in both cases, however, the effect upon the ear will be exactly the same, as a beating is no momentary phenomenon, but arises from the constant ebb and flow of the intensity of the note. To give a clearer idea of the order of vibrations in the beats of these harmonic intervals, I have reduced to writing the vibrations of the interval $n : hn$ and $n : hn + y$ ($h = 1, 2, \dots 8$) by means of my well-known apparatus, with which, according to the method first applied by Lissajous and Desains, one of the tuning-forks whose vibrations are to be calculated has attached to it a piece of smoked glass which vibrates with it, and the other carries the pencil which marks the figures upon this plate. If we look at the common characters of these figures, we find that the beats of the imperfect intervals $1 : 3$, $1 : 5$, $1 : 7$, as well as the beats of the unison, are shown by periodical maxima and minima of the amplitude of the vibrations which very clearly declare their direct audibility. In the perfect intervals, $1 : 2$, $1 : 4$, $1 : 6$, and $1 : 8$, a maximum of compression is constantly changing with a maximum of dilatation, as is the case in ordinary sound-waves, and every entire period may therefore be equally considered as a single united wave of air; and there can be nothing remarkable in such air-waves being considered singly as beats, as the notes of the great organ-pipe of the 32-foot octave may very easily be heard as single air-beats, and we receive the impression of a series of beats also if we apply the ear to the prongs of a large tuning-fork which gives less than 32 v. d.

Another peculiarity of the beats of harmonic intervals is that the two primary notes appear alternately. If at the same time with the powerful C we sound c only a small fraction of a vibration out of tune, so that very slow beats are formed, we hear the fundamental note and the octave alternately so clearly that, when c is very powerful, we should sometimes be inclined to count each vibration double; if, on the other hand, c is weak, we only hear the fundamental note becoming alternately stronger and weaker. I have succeeded in making precisely the same observations with the very slow beats of the twelfth and the double octave, $C:g$ and $C:c'$; but when the vibrations are at all quick the periodical appearance of the higher notes is no longer perceptible.

These phenomena also are more easily explained by means of beats of these intervals than by the supposition of resultant intermediate notes which cannot be heard. In the beat of the octave and the twelfth the fundamental note alone appears at a , and at b the higher note is distinguishable.

B. Intervals with the fundamental note c ($=256$ v. s.).

If we form the different intervals from the unison to the third octave with c ($=256$ v. s.) for the fundamental note, the beats of the different periods being twice as numerous, can no longer be observed with such wide intervals as were possible with the fundamental note C .

The first single audible beats change to a simple roll at the second, and to a confused rattle at the third, which, after the fourth, becomes feeble. Between the fifth and sixth the notes form a rough clang, through which, between the sixth and seventh, a more distinct roll begins to appear, which at the seventh changes to single perceptible beats, and at 496 v. s. (8 beats) to single computable beats, which disappear at the octave $c:c'$.

In the second period, $c:c'$ to $c:g'$, even at 584 v. s. only a roughness can be perceived; and at 608 v. s. the two notes already form a completely undisturbed clang, which only at 704 v. s. again becomes rough, and at 720 v. s. changes to a roll, that then melts into the single beats, which disappear at the twelfth, $c:g'$ (1:3).

In the third period, of $c:g'$ to $c:c''$ the last traces of the roughness produced by the increasing numbers of the lower beats disappear so soon as at 820 v. s. The two notes form, from this point to 976 v. s., an undisturbed simultaneous sound, which at 984 v. s. (20 beats, m') become rough, and then again allows the single beats to be heard, which disappear at the double octave, $c:c''$ (1:4).

Above the double octave we can distinguish below and above the interval $c : e''$ (1 : 5) the upper beats of the fourth and the lower beats of the fifth period to about the number of twelve. Above and below $c : g''$ (1 : 6) about 8 beats can be distinguished, and about 6 in the disturbed clang $C : 1792$ v. s. (1 : 7). The triple octave $c : e'''$ (1 : 8), when disturbed, allows 4 beats to be distinctly heard ; but the two or three beats perceptible at $c : d'''$ (1 : 9) are very weak.

Although both the lower and upper beats attain to the number of 64 in the interval with the fundamental note c , which forms the middle of that period, C is only very faintly distinguishable even in the first period in the fifth, $c : g$. If we suddenly produce a g beside the original singly sounding c , the result gives the same impression as if the fundamental note had acquired a deeper character.

C. Intervals with the fundamental note c' (=512 v. s.).

If intervals are formed with the fundamental note c' (=512 v. s.), gradually rising from the unison, the following phenomena will be noticed.

The first single audible lower beats change to a rattle before the second is reached, and at the third (64 beats) become a mere roughness ; at the same time a weak C is heard. At the fifth this note rises to c (128 beats), while at 720 to 736 v. s. the roughness of the clang is no longer heard. From 768 to 896 v. s. (128 to 192 beats) the note c rises to g , and is remarkably strong in proportion to its intensity between C and c (64 to 128 beats). It appears, therefore, that what the single impulses m have lost in intensity in these greater intervals, is fully made up by their greater number with regard to the intensity of the note which they form. The note produced by the upper beats m' can be distinguished by the beats of the auxiliary fork from the third (192 beats) to the fifth (128 beats), while it sinks from g to c , though it otherwise is scarcely audible. From 808 to 896 v. s. (108 to 64 beats m'), it becomes so feeble that even with the aid of the auxiliary fork it can scarcely be distinguished. It appears therefore that the increase in intensity of the single impulse m' , which is attained by the diminution of their number, is not great enough to form the deepened note with the same intensity which it possessed when it was higher. Towards 944 v. s. (40 beats m') a roughness arises, which at 976 v. s. changes to a roll that alters to single beats, which at the octave $c' : c''$ disappear.

The lower beats of the second period, from $c' : c''$ to $c' : g''$ (1 : 2 to 1 : 3), become at 20 only a roughness ; and in the

same way the upper beats at about 18 begin to be distinguishable by the roughness of the clang.

In the third period, from $c' : g''$ to $c' : c'''$, about 16 beats m can be heard, and about 10 beats m' .

(I made these two observations upon my tonometer with tuning-forks which are not mentioned in the list given in my introduction.)

Below and above the interval $c' : e'''$ (1 : 5) about 5 beats are easily heard, and in the interrupted interval $c' : g'''$ (1 : 6) from 2 to 3 can be distinguished.

The beat-notes, which in the first period were already extremely feeble, in the higher one are no longer directly perceptible.

D. Intervals with the fundamental note c'' (=1024 v. s.).

In intervals with the fundamental note c'' , the lower and upper beats are only to be distinguished as such near the unison and the harmonic interval. In consequence of their great numbers they change to notes which in the different intervals will be heard in the following manner :—

In the second, $c'' d''$, the note m (64 beats) C is distinctly heard ; in the third, $c'' e''$, it has risen to c (128 beats), and is still distinct ; in the fourth the note m (170.6 beats) f is joined by the note m'' (341.3 beats) f' . These two notes blend, when the fourth is quite pure, into a clang that is heard sometimes as f , and sometimes as f' . The notes m and m' become equal at the fifth $c'' g''$, when c' is very distinctly heard. At the sixth, the lower note m rises to f' , and the note m' sinks to f . These two notes are more powerful, and do not blend into one another so closely as at the fourth. If, with exactly the same intensity of the fundamental note the fork a'' is held a little further from the ear, the f is heard more strongly ; if it is brought nearer, f' becomes more distinct. In the interval $c'' : 1792$ v. s. (4 : 7) the two notes $m = g'$ and $m = e'$ are heard almost equally loud. At the seventh no more is distinguished of the lower note, and $m' = 64$ beats forms a mere rattle, a roughness through which C cannot be heard. Above the octave, in the interval $c'' : d'''$ (4 : 9), the note $m = 128$ beats, C , can be faintly heard, and in the interval $c'' : 2889.3$ v. s. (3 : 7), the note f . At $c'' : e'''$ (2 : 5), where $m = m' = 256$ beats, c' is very distinct ; beyond these limits no more beats can be distinguished ; only below and above the twelfth $e'' : g'''$ some distinct, and at the double octave a few very weak beats may be heard.

E. *Intervals with the fundamental note c''' (=2048 v. s.).*

If we now take c''' for the fundamental note of the interval, we arrive at that part of the scale which is especially adapted for the observation of the beat-notes, as the deepest octave was for the examination of the single beats which have not yet blended into one note.

The beat-notes of the first period may be heard in the following manner. The note c''' forms with

Interval $m\ m'$,

d'''	8: 9	c	—	m is heard alone and distinctly.
2389·3 v. s.	6: 7	f	—	m is heard alone and clearly.
e'''	4: 5	c'	g''	m is clear, m' weaker than m .
f'''	3: 4	f'	f''	m and m' melt into one sound.
2816 v. s.	8: 11	g'	e''	m and m' are equally loud.
g'''	2: 3	c''	e''	$m = m'$, the note is very loud.
3328 v. s.	8: 13	e''	g'	m and m' equally loud and distinct.
a'''	3: 5	f''	f'	m and m' louder than at the fourth, and also to be heard singly.
3584 v. s.	4: 7	g''	c'	m and m' about equally loud and distinct.
b'''	8: 15	—	c	m quite inaudible, m' audible and distinct.

In the second period, $c''' : c^{IV}$ to $c''' : g^{IV}$, the beat-notes are heard in the following manner:— c''' with

Interval $m\ m'$,

d^{IV}	4: 9	c'	g''	m distinctly audible, m' scarcely perceptible.
e^{IV}	2: 5	c''	c'	$m = m'$ distinctly audible.
f^{IV}	3: 8	f''	f'	m and m' about equally loud.
5632 v. s.	4: 11	g''	c'	m very faint, m' more distinctly audible than m .

Third period, from $c''' : g^{IV}$ to $c''' : c^V$.

6656 v. s.	4: 13	c'	—	m only audible.
a^{IV}	3: 10	f'	f''	m melts into m' .
7168 v. s.	2: 7	c''	c'	$m = m'$, distinctly.
b^{IV}	4: 15	—	c'	m' only audible.
7936 v. s.	8: 31	—	c	m' only audible.

F. *Intervals with the fundamental note c^{IV} (=4096 v. s.).*

Lastly, intervals with the fundamental-note c^{IV} allow the following notes to be heard:—

c^{IV} ,with interval $m\ m'$,

d^{IV}	8: 9	c'	—	m is distinctly perceptible.
e^{IV}	4: 5	c''	—	m is loud.
f^{IV}	3: 4	f''	—	m is equally loud.
5632 v. s.	8: 11	g''	e'''	m and m' loud.
g^{IV}	2: 3	c'''	c''	$m=m'$ quite loud.
6656 v. s.	8: 13	e'''	g''	m and m' both audible.
a^{IV}	3: 5	f'''	f''	m and m' audible.
7168 v. s.	4: 7	g'''	c''	m audible, m' louder than m .
b^{IV}	8: 15	—	c'	m quite inaudible, m' distinctly perceptible.
7936 v. s.	16: 31	—	c'	m' only distinctly audible.
8064 v. s.	32: 63	—	c	m' perceptible.

If the entire series of observations here set forth with their results be reviewed, it will be found that, taken as a whole, they show as follows:—

(1) The lower beats m , as well as the upper beats $m'=n-m$ of an interval $n:hn+m$ ($h=1, 2, 3\dots$), when the number of beats and the intensity of the primary notes are sufficient, change into beat-notes; for example the notes of the ratio 8:15, C:b, allow $m'=8$ beats to be heard, and the notes of the same ratio $c''' : b'''$ the beat-note $m'=c$, the notes $c^{IV} : b^{IV}$ the beat-note c' . Further, with the notes of the ratio 4:15 ($n:3n+m$), C:b, a distinct roll of 16 upper beats is heard, and with the notes of the same ratio, $c''' : d^{IV}$, the upper beat-note $m'=c'$.

(2) The beat-notes in the high octaves, and the singly audible beats in the low ones, are always equal to the two differences of the double vibrations of the higher primary notes, and of the two upper and lower notes of the harmonic series lying next above and below the deeper primary note, and not, as has been hitherto assumed, simply equal to the difference of the double vibrations of the two primary notes. For example, the notes of the ratio 4:9, $c''' : d^{IV}$ allow the beat-note $m=1=c'$, and no trace of the note $9-4=5=e'''$, to be heard; $c''' : e^{IV}$ (2:5) gives $m=1=c''$, and no trace of g''' . The ratio $n:2n+m$, 4:11, formed by the notes 2048 (c''') and 5632 v. s., allows further the beat-notes $m=3=g''$ and $m'=1=c'$ to be perceived, and no trace of the note $7=3584$ v. s.

(3) Of the beat-notes of the higher octaves m and m' , as well as of the singly audible beats m and m' of the lower ones, m alone is audible when m is much less than $\frac{n}{2}$, m' when

m is much greater than $\frac{n}{2}$, and the coexistence of m and m' is observable when m approaches $\frac{n}{2}$. For example, $c^{\text{IV}} : d^{\text{IV}}$ (8 : 9) only allows $m=1=c'$ to be heard; $c^{\text{IV}} : b^{\text{IV}}$ (8 : 15) only $m'=1=c'$; and at $c^{\text{IV}} : 6656$ v. s. (8 : 13) both $m=5=e''$ and $m'=3=g''$ are to be heard.

II. Secondary Beats and Beat-notes.

In the foregoing section I have endeavoured to describe connectedly the operations of the upper and lower beats, as they appear in the different intervals when these are formed, first from the deepest notes, then from higher and higher up to highest, and so as not to disturb their connexion. There yet remains a class of phenomena which I will now describe.

We have seen above that in the clang of the two notes 80 and 148 v. s., the roll of the 34 lower beats m , and the single audible 6 upper beats m' can be separately heard, that in the neighbourhood of the fifth C : G a strong confused rattle is caused by the coexistence of these two kinds of beats, and that finally in the high octaves, as also in the intervals $n : hn + m$, if m approaches to $\frac{n}{2}$, both the beat-notes m and m' can be observed together. These two beat-notes, which appear side by side, are in the same relation to each other as would be the case with two equal primary notes of the same intensity: *i. e.* if they are near the unison they allow strong beats to be heard; if they form almost the interval of an octave they also produce beats, which, however, are weaker; and in the same way their broken twelfth will also allow beats to be heard.

In the intervals $n : hn + m$ the two beat-notes m and m' are in unison if $m = \frac{n}{2}$, therefore in the intervals 2 : 3, 2 : 5, 2 : 7.

If, however, $m = \frac{n}{2} + 1$, then $n - m = \frac{n}{2} - 1$, and we obtain two beats.

The upper beat-note m' is the higher octave of the lower beat-note m if $m = \frac{n}{3}$, and therefore in the intervals 3 : 4, 3 : 7....

If, however, $m = \frac{n}{3} + 1$, then $n - m = \frac{2n}{3} - 1$, and we obtain

$$\left(\frac{2n}{3} + 2\right) - \left(\frac{2n}{3} - 1\right), \text{ i. e. three beats.}$$

The lower beat-note is the higher octave of the upper beat-

note if $m = \frac{2n}{3}$, and therefore in the intervals $3 : 3$, $3 : 8$. If, however, $m = \frac{2n}{3} + 1$, then $n - m = \frac{n}{3} - 1$, and we again obtain $\left(\frac{2n}{3} + 1\right) - \left(\frac{2n}{3} - 2\right)$, *i. e.* three beats.

The beat-notes m and m' together form the twelfth if $m = \frac{n}{4}$, in the intervals $2 : 5$, $4 : 9$; and if $m = \frac{3n}{4}$, in the intervals $4 : 7$, $4 : 11$. If $m = \frac{n}{4} + 1$, then $m' = \frac{3n}{4} - 1$, and we obtain $\left(\frac{3n}{4} + 3\right) - \left(\frac{3n}{4} - 1\right)$, *i. e.* four beats.

In general, then, when the higher note deviates by a double vibration from the perfect interval, there ensue two beats in the intervals $2 : 3$, $2 : 5$, $2 : 7$, three beats in the intervals $3 : 4$, $3 : 7$, and $3 : 5$, $3 : 8$, and finally four beats in the intervals $4 : 5$, $4 : 9$, and $4 : 7$, $4 : 11$.

By the use of the loud notes at my disposal I was able to make the following observations on these secondary beats arising from beat-notes.

Near the fifth double E and double B, where the primary notes make a distinct rattle, only one or two secondary beats are audible; at the fifth double G : D (96 : 144 v. s.), where also the primary beats make a distinct rattle, but where, in consequence of the greater intensity of the primary notes, they are much louder, the secondary beats to the number of 8, and above the fifth to the number of 10, can be followed; in fact, above the fifth they are more distinct, as is also the case in the higher positions,—which may be explained by the fact that in this neighbourhood the intensity of the lower and upper beats must be more nearly equal, because the upper beats, which are weaker even when their number is the same, are not here so numerous as the lower beats m , whilst below the fifth the contrary is the case. With equal intensity of the fundamental note, the secondary beats are most distinct when the higher note is somewhat weaker, while the rattle of the primary beat is loudest when the higher notes are the stronger.

In the interval with the fundamental note C, I was only able to observe the secondary beats in the interrupted unison m and m' , but there as far as the third period. They may be perceived at C : G (2 : 3) to the number of 6 or 8, and at C : e (2 : 5) to the number of 5 or 6. At 2 : 7 two or three may be heard.

In the intervals double E : double B, double G : D, and C : G,

the secondary beats sound in conjunction with the loud rattle of the primary beats somewhat in the manner that I have described above in the simultaneous sounding of 80 : 144 v. s. At $C : e$, however, where the rattle of the primary beats is already much weaker, it disappears before the secondary beats; and the same thing occurs at the fifth $c : g$.

In the intervals with the fundamental note c , the whole system of secondary beats can be very fully observed. The beats of the unison of the beat-notes can not only be numerous and clearly heard in the intervals 2 : 3, where they can be followed till they change to a rattle of from 12 to 16, at 2 : 5, 2 : 7, and even at 2 : 9 to the number of about 4, but also in the octave formed by m and m' at 3 : 4, 3 : 5 to about 6 or 8, at 3 : 7 and 3 : 8 (the former weaker than the latter) to about 4, and at 3 : 11 in the third period to 3 or 4. The beats of the twelfth of m and m' are only perceptible in the first period at the intervals 4 : 5 and 4 : 7, and can only be followed to about 3 or 4.

In the intervals with the fundamental note c' the vibrations of my tuning-forks are somewhat less favourable than in those just mentioned with the fundamental note c . Consequently the secondary beats at the unison of the beat-notes m and m' in the first three periods were really quite distinctly audible in the intervals 2 : 3, 2 : 5, and 2 : 7; and when they formed together the octave, in the first period only, at 3 : 4 and 3 : 5.

In the first period of the interval with the fundamental note c'' , the secondary beats can be perceived in all intervals in which the beat-notes stand in the ratio of 1 : 1, 1 : 2, and 1 : 3 to one another; in the second period, however, only a few distinct beats at 2 : 5 and some very weak ones at 3 : 7 can be distinguished.

Intervals with the fundamental note c''' are formed in the first period by a powerful tuning-fork for the fundamental note, and weaker forks for the upper notes. Here the secondary beats are only heard clearly at 2 : 3, and further at 3 : 4 and 3 : 5. Above the octave, however, with the powerful forks of the octave c^{IV} to c^V , the beats of the beat-notes are heard not only at 2 : 5 and 2 : 7 and at 3 : 8, but even at 4 : 9.

Experiments on all these intervals formed from very high notes are already very fatiguing to the ear; and this is still more the case in the intervals of the octave of c^{IV} to c^V . I succeeded, however, in distinguishing besides the secondary beats of the fifth, and the fourth and sixth, also those of the third and of the ratio 4 : 7. The extraordinary intensity of the notes of my forks for this octave proved itself especially valuable in the intervals 8 : 11 and 8 : 13.

As I have already stated, the simultaneous sound of 4096 (c^{IV}) and 5632 v. s. (8:11) allows $m=768$ beats (g'') and $m'=1280$ beats (e''') to be distinctly heard, besides which a quite distinct c'' , which is $=512$ v. d., that is, $=1280-768$ v. d., may be perceived; and the same result is obtained by a simultaneous sound of 4096 and 6656 v. s. (8:13) when $m=1280$ and $m'=768$ beats. The note c'' may also be clearly perceived here; so that the secondary beats, if their number and strength are sufficient, melt into one note like the primary beats.

I only observed the secondary beat-notes in these two cases; but there they were quite clear and distinct. In the deeper octave, where the same intervals produce the distinctly audible beat-notes g' and e'' , the latter, in consequence of the greater weakness of the primary notes, does not allow the c' , which ought to be there, to be heard.

With regard to the general observation of the secondary beats, it may be remarked that the weaker they are, the less must they exceed a certain number if they are to be clearly distinguished; it must not, therefore, be forgotten when the higher primary note is put out of tune in order to bring them out, that if this note is put out of tune by one double vibration, 2, 3, or 4 secondary beats are produced. In the interval $c:e$ for instance, therefore, the latter note must only be put out of tune by one double vibration at the outside if the secondary beats are to be clearly perceptible; otherwise nothing more is heard of them; at least when I have struck these notes together I have never distinguished more than four. At the simultaneous sound of c^{IV} and e^{IV} the secondary beats are also most distinctly heard when they are about four in number. My e^{IV} fork weighs about 560 grammes; and even a little lump of wax weighing about a decigramme attached to the end of one of its prongs puts it out of tune to the extent of a double vibration, and thereby allows the four secondary beats to be heard. From this example we may see how easily it may often happen that the secondary beats cannot be perceived merely because the interval of the two primary notes is too much out of tune.

I have already remarked, while on the subject of pure harmonic intervals, that until now all beats of wider intervals have been traced back to the beats of two notes near the unison. It was supposed that the first difference note of the primary notes again produced difference notes with these primary notes, that these produced others with the primary notes and the first difference note, and so it was continued until two of the notes near the unison were reached, which would then

be sounded together. Suppose, for example, that in the disturbed major third $4n : 5n + x$ there appeared,

$$\begin{array}{rcl}
 5n + x - 4n & = & n + x \\
 4n - (n + x) & = & 3n - x \\
 5n + x & & - (3n - x) = 2n + 2x \\
 4n & & - (2n + 2x) = 2n - 2x.
 \end{array}$$

and then $2n + 2x$, with $2n - 2x$, allow $4x$ beats to be heard. By this proceeding we always arrive at the true number of beats; but we are obliged at the same time always to suppose the existence of notes which are not only themselves unheard, but are also often both the effect of some and the cause of other notes, which are all equally inaudible. In the example here given, $5n + x$ and $4n$ show the beat-note $n + x$ at a certain intensity; but if we now sound a primary note $n + x$ of about equal intensity and, at the same time, alone with the primary note $4n$, we shall only hear $4x$ beats, but under no circumstances a note $3n - x$ of such intensity that it could be in a position to produce other notes again in fresh combinations. This note, $3n - x$, to judge by the analogy in other cases, would not be strong enough for this, even if it were a beat-note; but it has been produced from $n + x$ and $4n$, and is therefore only a difference note; and how far below the beat-notes in intensity are the difference notes and summation notes we shall see later, in the section that treats of these notes.

With how much caution we must regard an explanation of the wider intervals of the beats by the combination notes becomes still more obvious if, instead of an interval of the first period, we examine a simultaneous sound of the second or third. We have seen above that distinct secondary beats can be heard in the ratio $2 : 7$. If this is formed with the fundamental note c''' , both the beat-notes m and $m' = c''$, and this c'' we can hear loudly and distinctly. If the interval $2 : 7$ is less out of tune, and m and m' consequently no longer in clear unison, they sound together in precisely the same way as two primary notes c'' of the same intensity would do if put out of tune to the same extent, and we need no further inaudible note for the explanation of this phenomenon; but according to the old view,

$$\begin{array}{rcl}
 7n + x - 2n(c''') & = & 5n + x(c^{iv} + x) \\
 & & 5n + x - 2n = 3n + x(g''' + x) \\
 7n + x & & - (3n + x) = 4n(c^{iv}) \\
 & & 5n + x & - 4n = n + x(c'' + x) \\
 & & & 4n - (n + x) = 3n - x(g''')
 \end{array}$$

and consequently $3n+x$ and $3n-x$ would give the beats $2x$. Nothing, however, can be discovered of all these intermediate notes; and we may well suppose therefore that, if with such extraordinarily strong notes as I have used there is hardly any probability of secondary beats being produced by combination notes, then with the use of weaker simple notes (such as, for example, those produced by organ-pipes) the supposition seems to be deprived of all probability whatever. But if, on the other hand, we succeeded in producing such powerful simple primary notes that the combination notes necessary for the formation of the secondary beats according to the old view could be formed with sufficient intensity, even in this case the two beat-notes (m and m') and their beats might have attained to such strength that the combination notes of the higher order, falling together with the latter beats, might still form only an extremely small part of the intensity of the beats heard.

In order to allow an easy review of all my observations on primary and secondary notes and beat-notes, I have drawn up the following Table. The column A contains the primary notes with their vibrations, B the ratio of these two notes, C the number of lower beats m , c the ratio they form with the fundamental note of the interval, D the number of the upper beats m' , and d the ratio they form with the fundamental note. Under E is stated how the lower beats m , and under F how the upper beats m' are to be heard. Finally, the column G contains the secondary beats and secondary beat-notes arising from the combined action of m and m' . In this Table I have only given such results as may be perceived by any ordinarily good ear from the use of the notes which I have employed in these experiments; and I have noticed especially the cases in which notes cannot be directly distinctly heard, whose undoubted existence is proved not only by secondary beats, but also by the help of auxiliary forks, as is, for example, the case in the beat-notes of the intervals $c':e'$ and $c':f'$. An "ordinarily good ear," and "notes such as I have employed," are certainly, in spite of the dimensions of the tuning-forks and sounding-boxes stated above, premises much wanting in precision; but it stands to reason that even the phenomena resulting from the simultaneous sound of two simple notes can only be stated with perfect accuracy in relation to their intensity when it becomes possible to express the intensity of notes of different pitches by a common ratio, with the same precision with which we can now state the pitch of their vibrations.

A few apparent anomalies which are shown by this Table, as, *e. g.*, that the system of secondary beats can be less perfectly

observed in the intervals with the fundamental note c' than in those with the fundamental notes c and c'' , and the absence of beat-notes in the intervals with the fundamental note c'' which lie above 2:5, may be explained, as I stated above, by the lesser intensity of the notes which formed the intervals in question.

TABLE of the Primary and Secondary Beats and Beat-notes observed directly.

Intervals with the fundamental note Double E(=80 v. s.).

A.	B.	E.	c .	C.	G.	D.	d .	F.
v. s.	$n:n+m$			m		m'		
Db. E : Db. E = 80	1:1	Unison.	...	0				
" : Db. G = 100	4:5	Singly audible.	...	10	0-2	28.6		
" : Db. A = 106.6	3:4	Loud rattle.	...	13.3		20	1	
" : Db. B = 120	2:3	"	...	20	X	8		
" : " 144	"	...	32	0-2	6		Appear.
" : " 148	"	...	34		5		Distinct
" : " 150	Weaker rattle.	...	35	2		"
" : " 156	38	0		"
" : " 160			Octave.

Intervals with the fundamental note Double G(=96 v. s.).


Db. G : Db. G = 96	1:1	Unison.	...	0				
" : " 116	Singly audible.	...	10	0-8	28		Loud ra
" : " 136	Loud rattle.	...	20		26		"
" : " 140	22	X	24	1	"
" : Db. D = 144	2:3	24		22		"
" : " 148	26		20		"
" : " 152	28	0-8	18		"
" : " 156	30		16		"
" : Db. E = 160	3:5	32	10		Singly 8
" : " 172	38	0		Octave.
" : " 192			

Intervals with the fundamental note C(=128 v. s.).

First period of C:C (1:1) to C:c (1:2).

v. s.	$n:n+m$			m		m'	
C:C=128	1:1	Unison.	...	0			
132	Singly audible.	...	2			
136	"	...	4			
140	"	...	6			
C:D=144	8:9	"	...	8			
148	"	...	10			
152	"	...	12			
156	Simple roll.	...	14			
C:E=160	4:5	"	...	16			
164	"	...	18			
168	"	...	20			
C:F=170.6	3:4	"	...	21.3			
172	"	...	22			

Table (continued).

A.	B.	E.	c.	C.	G.	D.	d.	F.
v. s.	$n : n + m$			m		m'		
176	Confused rattle.	...	24	40	...	Confused
180	"	...	26	0-8	38	...	rattle.
184	"	...	28		36	...	"
188	"	...	30		34	...	"
C: G=192	2:3	"	1	32		32	...	"
196	"	...	34	0-8	30	...	"
200	"	...	36		28	...	"
204	"	...	38		26	...	"
208	"	...	40	24	...	"
212	"	...	42	22	...	"
C: A=213.3	3:5	"	...	42.6	20.3	...	"
216	44	20	...	Simple roll.
220	18	...	"
224	4:7	16	...	"
228	14	...	"
232	12	...	"
236	10	...	Singly audi-
C: B=240	8:15	8	...	ble.
244	6	...	"
248	4	...	"
252	2	...	"
C: c=256	1:2	0	...	Octave.

Second period, from C : c (1 : 2) to C : g (1 : 3).


v. s.	$n : 2n + m$			m		m'		
C: c=256	1:2	Octave.	...	0				
260	Singly audible.	...	2				
264	"	...	4				
268	"	...	6				
272	"	...	8				
276	"	...	10				
280	Simple roll.	...	12				
284	"	...	14				
C: d=288	4:9	"	...	16				
292	"	...	18				
296	Confused rattle.	...	20	44	...	Confused
300	"	...	22	42	...	rattle.
304	"	...	24	40	...	"
308	Weak rattle,	...	26	0-6	38	...	"
312	which disap-	...	28		36	...	"
316	pears before	...	30		34	...	"
C: e=320	2:5	the secondary	1	32		32	1	"
324	beats.	...	34	0-6	30	...	"
328	"	...	36		28	...	"
332	"	...	38		26	...	"
336	Mere rough-	...	40	24	...	Mere rough-
340	ness.	22	...	ness.
C: f=341.3	3:8	21.3	...	"
344	20	...	"
348	18	...	Simple roll.
352	16	...	"
356	14	...	"
360	12	...	"
364	10	...	Singly audi-
368	8	...	ble.
372	6	...	"
376	4	...	"
380	2	...	"
C: g=384	1:3	0	...	Twelfth.

Table (*continued*).Third period, of $C : g$ (1 : 3) to $C : c'$ (1 : 4).

A.	B.	E.	c.	C.	G.	D.	d.	F.
v. s.	$n : 3n + m$			m		m'		
$C : g = 384$	1 : 3	Twelfth.	...	0				
388	Singly audible.	...	2				
392	"	...	4				
396	"	...	6				
400	"	...	8				
404	Simple roll.	...	10				
408	"	...	12				
412	"	...	14				
416	"	...	16				
420	Confused and	...	18	Confused
424	weak rattle.	...	20	weak ra
426·6	"	...	21·3	
428	"	...	22	
432	"	...	24	40	...	
436	"	...	26	38	...	
440	"	...	28	36	...	
444	"	...	30	0-3	34	...	
448	2 : 7	"	1	32	X	32	...	
452	"	...	34	0-3	30	...	
456	Mere rough-	...	36	28	...	Mere rou
460	ness.	26	...	ness.
464	24	...	"
468	22	...	"
472	20	...	"
476	18	...	"
480	16	...	"
484	14	...	"
488	12	...	"
492	10	...	Singly au
496	8	...	ble.
500	6	...	"
504	4	...	"
508	2	...	" [t
512	1 : 4	0	...	Double

Fourth period, from $C : c'$ (1 : 4) to $C : e'$ (1 : 5).

v. s.	$n : 4n + m$			m		m'		
$C : c' = 512$	1 : 4	Double octave.	...	0				
520	Singly audible.	...	4				
528	"	...	8				
536	Simple rattle.	...	12				
544	Faint rattle.	...	16				
552	Pure simulta-	...	20				
560	neous sound.	...	24				
568	"	...	28				
$C : d' = 576$	2 : 9	"	...	32				
...				
616	10	...	Perceptib
624	8	...	Singly cle
632	4	...	
$C : e' = 640$	1 : 5	1 : 5	0	...	Third of double c

Table (continued).

Fifth period, from $C : e'$ (1 : 5) to $C : g'$ (1 : 6).

A.	B.	E.	<i>c.</i>	C.	G.	D.	<i>d</i>	F.
v. s. $C : e' = 640$	$n : 5n + m$ 1 : 5	Third of double octave.	...	m 0		m'		
648	Singly audible.	...	4				
656	"	...	8				
660	Still clear.	...	10				
664	Disappear.	...	12				
...								
748	10	...	Appear.
752	8	...	Singly clear.
760	4		
$C : g' = 768$	1 : 6	0	...	Fifth of the double oct.

Sixth period, from $C : g'$ (1 : 6) to $C : 896$ v. s. (1 : 7).

v. s. $C : g' = 768$	$n : 6n + m$ 1 : 6	Fifth of the double octave.	...	m 0		m'		
776	Singly audible.	...	4				
780	Still distinct.	...	6				
784	Disappeared.	...	8				
...								
884	6	...	Faintly aud.
888	4	...	Quite di-
892	2	...	stinct.
896	1 : 7	0	...	Pure 1 : 7.

Seventh period, from $C : 896$ v. s. (1 : 7) to $C : c''$ (1 : 8).

v. s. $C : 896$	$n : 7n + m$ 1 : 7	Pure 1 : 7.	...	m 0		m'		
900	Singly audible.	...	2				
904	Still distinct.	...	4				
...								
1016	Disappeared.	...	6	4	...	Perceptible.
1020	2	...	Quite di-
1024	1 : 8	0	...	stinct. Third octave.

Intervals with the fundamental note c (=256 v. s.).First period, from $c : c$ (1 : 1) to $c : c'$ (1 : 2).

v. s. $c : c = 256$	$n : n + m$ 1 : 1	Unison.	...	m 0		m'		
264	Singly audible.	...	4				
272	"	...	8				
280	"	...	12				
$c : d = 288$	8 : 9	Simple rattle.	...	16				
296	"	...	20				
304	"	...	24				
312	"	...	28	0-4	100		
$c : e = 320$	4 : 5	Confused rattle.	1	32	> <	96	3	
328	"	...	36	0-4	92		
336	"	...	40	0-6	88		
$c : f = 341.3$	3 : 4	"	1	42.6	X	85.3	2	
344	"	...	44		84		
352	"	...	48	0-6	80		

Table (continued).

A.	B.	E.	c	C.	G.	D.	d.	F
v. s	$n : n + m$			m		m'		
360	The rattle gets	...	52	76		
368	gradually	...	56	0-8	72		
376	fainter, and	...	60	\times	68		
$c : g = 384$	2 : 3	disappears	...	64	\times	64	...	Rough
392	before the se-	...	68	0-10	60	...	(C se
400	condary beats.	...	72		56	...	audib
408	76	52
416	80	0-6	48
424	84	\times	44
$c : a = 426 \cdot 6$	3 : 5	2	85·3	\times	42·6	1	..
432	88	0-8	40
440	92	0-4	36	...	More d
448	4 : 7	3	96	$> <$	32	1	roll.
456	0-4	28
464	24
472	20
$c : b = 480$	8 : 15	16	...	Singly
488	12
496	8
504	4
$c : c' = 512$	1 : 2	0	...	Octave

Second period, from $c : c' (1 : 2)$ to $c : g' (1 : 3)$.

v. s.	$n : 2n + m$			m		m'		
$c : c = 512$	1 : 2	Octave.	...	0				
520	Singly audible.	...	4				
528	"	...	8				
536	"	...	12				
544	Simple roll.	...	16				
552	"	...	20				
560	"	...	24				
568	"	...	28				
$c : d' = 576$	4 : 9	Faint roll.	...	32				
584	Roughness.	...	36		92		
592	"	...	40·6	0-3	88		
596·3	"	1	42	\times	85·3	2	
600	"	...	44	0-3	84		
608	Undisturbed	...	48		80		
616	simultaneous	...	52	76		
624	sound.	...	56	0-8	72		
632	60	\times	68		
$c : e' = 640$	2 : 5	1	64	\times	64	1	
648	68	0-10	60		
656	72	56		
664	76	52		
672	80	48		
680	84·3	0-4	44		
$c : f = 682 \cdot 6$	3 : 8	2	85	$> <$	42·6		
688	88	0-4	40		
696	36		
704	32	...	Rough
712	28	...	Powerf
720	24	...	Roll.
728	20	...	"
736	16	...	"
744	12	...	Singly
752	8	...	"
760	4	...	"
$c : g' = 768$	1 : 3	0	...	Twelfth

Table (continued).

Third period, from $c : g' (1 : 3)$ to $c : c'' (1 : 4)$.

A.	B.	E.	c.	C.	G.	D.	d.	F.
v. s. $c : g' = 768$	$n : 3n + m$ 1 : 3	Twelfth.	...	m 0		m'		
776	Singly audible.	...	4				
784	"	...	8				
792	Roll.	...	12				
800	"	...	16				
808	"	...	20				
816	Roughness.	...	24				
824	Undisturbed	...	32				
⋮		simultaneous	...	⋮				
888	sound.	...	60	0-8	68		
896	2 : 7	1	64	\times	64	1	
904	68	0-10	60		
912	72		56		
920	76		52		
928	80	0-4	48		
936	84	$> <$	44		
944	1 : 11	2	88	0-4	40	1	
⋮						⋮		
984	20	...	Roughness.
992	16	...	Greater roughness.
1000	12	...	Roll.
1008	8	...	Singly aud.
1016	4	...	"
$c : c'' = 1024$	1 : 4	0	...	Double oct.

Fourth period, from $c : c'' (1 : 4)$ to $c : e'' (1 : 5)$.

v. s. $c : c'' = 1024$	$n : 4n + m$ 1 : 4	Double octave.	...	m 0		m'		
1032	Singly audible.	...	4	0-4			
⋮								
1148	Undisturbed	...	60	\times	68	1	
$c : d'' = 1152$	simultaneous	1	64	0-4	64		
1160	sound.	...	68		60		
1272		4	...	Singly aud.
$c : e'' = 1280$	1 : 5		0	...	Third of the double oct.

Fifth period, from $c : e'' (1 : 5)$ to $c : g'' (1 : 6)$.

v. s. $c : e'' = 1280$	$n : 5n + m$ 1 : 5	Third of the	...	m 0		m'		
1304	double octave.	...					
⋮		Singly audible.	...	12				
1520		6	...	Audible.
$c : g'' = 1536$	1 : 6		0	...	Fifth of the double oct.

Sixth period, from $c : g''$ (1 : 6) to $c : 1792$ v. s. (1 : 7).

A.	B.	E.	c.	C.	G.	D.	d.	F.
v. s. $c:g''=1536$	$n:6n+m$ 1:6	Fifth of the	...	m 0		m'		
⋮		double octave.	...	⋮				
1552	Audible.	...	8				
⋮								
1780	6	...	Audible.
1792	1:7	0	...	Pure 1:7

v. s.	$n:7n+m$		m		m'	
$c:1792$	$1:7$	Pure 1:7.	0			
1804	Audible.	6			
2040		4	Audible.
$c:c'''=2048$	$1:8$	0	Third oct

$v. s.$	$n : n + m$		m		m'	
$c' : c' = 512$	1 : 1	Unison.	...	0		
528	Beats.	...	8		
544	Roll.	...	16		
560	Rattle.	...	24		
$c' : d' = 576$	8 : 9	"	...	32		
592	"	...	40		
608	"	...	48		
624	"	...	56	0-4	
$c' : e' = 640$	4 : 5	Roughness and	1	64	$> <$	192
656	soft note C.	...	72	0-4	184
672	"	...	80	0-8	176
$c' : f' = 682.6$	3 : 4	"	1	85.3	\times	170.6
688	Note without	...	88		168
704	roughness.	...	96	0-8	160
720	"	...	104	152
736	"	...	112		144
752	"	...	120	0-16	136
$c' : g' = 768$	2 : 3	"	...	128	\times	128
784	"	...	136		120
800	Note rather	...	144	0-16	112
816	louder.	...	152	104
832	"	...	160	0-6	96
848	"	...	168		88
$c' : a' = 853.3$	3 : 5	"	2	170.6	\times	85.3
864	Note again	...	176	0-6	80
880	fainter, but	...	184		72
896	still perceptible	...	192	64
912	with auxiliary	56
928	forks.	48
944	40
$c' : b' = 960$	8 : 15	32
976	24
992	16
1008	8
$c' : c'' = 1024$	1 : 2	0

Table (continued).

Second period, from $c' : c''$ (1 : 2) to $c' : g''$ (1 : 3).

A.	B.	E.	c.	C.	G.	D.	d.	F.
v. s.	$n : 2n + m$			$m.$		$m'.$		
$c' : c'' = 1024$	1 : 2	Octave.	...	0				
1032	Singly audible.	...	4				
1040	8				
⋮								
$c' : d'' = 1152$	4 : 9	Faintroughness	...	64				
⋮								
$c' : e'' = 1280$	2 : 5	1	128	0-4 > <	128	1	
⋮								
$c' : f'' = 1365.3$	3 : 8	170.6				
1496	20	...	Roughness.
1512	12	...	Singly aud.
1520	8	...	"
1528	4	...	"
$c' : g'' = 1536$	1 : 3	0	...	Twelfth.

Third period, from $c' : g''$ (1 : 3) to $c' : c'''$ (1 : 4).

v. s.	$n : 3n + m$			$m.$		$m'.$		
$c' : g'' = 1536$	1 : 3	Twelfth.	...	0				
1552	Singly audible.	...	8				
1578	Roughness.	...	16				
⋮				⋮				
1792	2 : 7	1	128	0-3 > < 0-3	128		
⋮								
2028	10	...	Rattle.
2032	8	...	Singly aud.
2040	4	...	"
$c' : c''' = 2048$	1 : 4	0	...	Double octave

Fourth period, from $c' : c'''$ (1 : 4) to $c' : e'''$ (1 : 5).

v. s.	$n : 4n + m$			$m.$		$m'.$		
$c' : c''' = 2048$	1 : 4	Double octave	...	8				
⋮		beats to about	...					
$c' : d''' = 2304$	2 : 9	Unbroken						
⋮		clang.						
5550	5	...	Singly aud.
$c' : e''' = 5560$	1 : 5	0	...	Third of the double octave

Fifth period, from $c' : e'''$ (1 : 5) to $c' : g'''$ (1 : 6).

v. s.	$n : 5n + m$			$m.$		$m'.$		
$c' : e''' = 5560$	1 : 5	Third of the	...	0				
⋮		double octave.	...					
5570	Singly audible.	...	5				
3066	3	...	Singly aud.
$c' : g''' = 3072$	1 : 6	0	...	Fifth of the double octave

Table (*continued*).Intervals with the fundamental note $c'' (= 1024 \text{ v. s.})$.First period, from $c'' : c'' (1 : 1)$ to $c'' : c''' (1 : 2)$.

A.	B.	E.	c .	C.	G.	D.	d .	F.
v. s. $c'' : c'' = 1024$ $c'' : d'' = 1152$	$n : n + m$ 1 : 1 8 : 9	Unison beats. Note C weak.	...	m . 0 64		m' .		
$c'' : e'' = 1280$	4 : 5	Note c strong.	1	128	Clearly × distinguishable	384	3	Note g'
$c'' : f'' = 1365 \cdot 3$	3 : 4	f strong,	1	170·6	× Very	341·3	2	f' blend with
$c'' : g'' = 1536$	2 : 3	Note c' quite strong.	1	256	× clear. Clearly	256	1	c' quite
$c'' : a'' = 1706 \cdot 6$ 1792	3 : 5 4 : 7	Note f' loud. Note g' distinguishable.	2 3	341·3 384	× distinguish- × able.	170·6 128	1 1	f loud. c distinguishable.
$c'' : b'' = 1920$	8 : 15	64	...	Rough C very beats.
$c'' : c''' = 2048$	1 : 2	0	...	Octave

Second period, from $c'' : c''' (1 : 2)$ to $c'' : g''' (1 : 3)$.

v. s. $c'' : c''' = 2048$ $c'' : d''' = 2304$	$n : 2n + m$ 1 : 2 4 : 9	Octave beats. c audible.	...	m . 128		m' .		
2389·3	3 : 7	f audible.	1	170·6	Hardly audible. × × Clear.	341·3	2	f blend with
$c'' : e''' = 2560$	2 : 5	c quite loud.	1	256		256	1	c' quite
$c'' : f''' = 2730 \cdot 6$ 2816	3 : 8 4 : 11						
$c'' : g''' = 3072$	1 : 3	Beats, Twelfth

Third period, from $c'' : g''' (1 : 3)$ to $c'' : c^{iv} (1 : 4)$.

v. s. $c'' : g''' = 3072$ $c'' : c^{iv} = 4096$	$n : 3n + m$ 1 : 3 1 : 4	Twelfth beats.	m . 0 :	m' . : 0	...	Beats. Double
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Table (continued).

Intervals with the fundamental note $c''' (= 2048 \text{ v. s.})$.First period, from $c''' : c''' (1 : 1)$ to $c''' : c^{IV} (1 : 2)$.

A.	B.	E.	<i>c.</i>	C.	G.	D.	<i>d.</i>	F.
v. s.	$n : n + m$			<i>m.</i>		<i>m'.</i>		
$c''' : c''' = 2048$	1 : 1	Unison beats.	...	0				
$c''' : d''' = 2304$	8 : 9	<i>c</i> quite audible.	...	128				
2389·3	6 : 7	<i>f</i> quite audible.	...	170·6				
$c''' : e''' = 2560$	4 : 5	<i>c'</i> loud.	1	256	Audible.	768	3	<i>g''</i> faint.
$c''' : f''' = 2730·6$	3 : 4	<i>f'</i> loud.	1	341·3	Distinct.	682·6	2	$\left\{ \begin{array}{l} f'' \text{ blended} \\ \text{with } f'. \end{array} \right.$
2816	8 : 11	<i>g'</i> loud.	...	384	$> <$	640	...	<i>e''</i> loud with <i>g'</i>
$c''' : g''' = 3072$	2 : 3	<i>c''</i> very loud.	1	512	Loud.	512	1	<i>c''</i> very loud.
3328	8 : 13	<i>e''</i> loud.	...	640	$> <$	384	...	<i>g'</i> loud with <i>e''</i>
$c''' : a''' = 3413·3$	3 : 5	<i>f''</i> distinct.	2	682·6	Distinct.	341·3	1	<i>f''</i> distinct.
3584	4 : 7	<i>g''</i> perceptible.	3	768	$> <$	256	1	<i>c'</i> perceptible.
$c''' : b''' = 3840$	8 : 15	Audible.	128	...	<i>c</i> distinct beats
$c''' : c^{IV} = 4096$	1 : 2	0	...	Octave.

Second period, from $c''' : c^{IV} (1 : 2)$ to $c''' : g^{IV} (1 : 3)$.

v. s.	$n : 2n + m$			<i>m.</i>		<i>m'.</i>		
$c''' : c^{IV} = 4096$	1 : 2	Octave.	...	0				
$c''' : d^{IV} = 4608$	4 : 9	<i>c'</i> loud.	1	256	Perceptible.	768	3	<i>g''</i> feeble.
$c''' : e^{IV} = 5120$	2 : 5	<i>c''</i> loud.	1	512	Loud.	512	1	<i>c''</i> loud.
$c''' : f^{IV} = 5461·3$	3 : 8	<i>f''</i> powerful.	2	682·6	$> <$	341·3	1	<i>f'</i> weaker.
5632	4 : 11	<i>g''</i> faint.	...	768	Perceptible.	256	...	<i>c'</i> quite aud.
$c''' : g^{IV} = 6144$	1 : 3	0	...	Beats, Twelfth.

Third period, from $c''' : g^{IV} (1 : 3)$ to $c^{IV} : c^V (1 : 4)$.

v. s.	$n : 3n + m$			<i>m.</i>		<i>m'.</i>		
$c''' : g^{IV} = 6144$	1 : 3	Twelfth.	...	0				
6656	4 : 13	<i>c'</i> audible.	...	256				
$c''' : a^{IV} = 6826·6$	3 : 10	<i>f'</i> audible.	...	341·3	682·6	...	<i>f''</i> blended with <i>f'.</i>
7168	2 : 7	<i>c''</i> distinct.	1	512	Audible.	512	1	<i>c''</i> distinct.
$c''' : b^{IV} = 7680$	4 : 15	$> <$	256	...	<i>c'</i> faint but distinct.
7936	8 : 31	128	...	<i>c</i> perceptible.
$c''' : c^V = 8192$	1 : 4	1	Double octave

Table (continued).

Intervals with the fundamental note c^{IV} (=4096 v. s.).

A.	B.	E.	c.	C.	G.	D.	d.	F.
v. s.	$n : n + m$			m .		m' .		
$c^{IV} : c^{IV} = 4096$	1 : 1	Unison beats.	...	256				
$c^{IV} : d^{IV} = 4608$	8 : 9	c' loud.	...	256				
$c^{IV} : e^{IV} = 5120$	4 : 5	c'' loud.	1	512	Audible.		3	
$c^{IV} : f^{IV} = 5461.3$	3 : 4	f''' loud.	1	682.6	$> <$...	2	
5632	8 : 11	g'' loud.	3	768	Distinct.			
$c^{IV} : g^{IV} = 6144$	2 : 3	c''' loud.	1	1024	e''	1280	5	e''' loud
6656	8 : 13	e''' loud.	5	1280	Loud.	1024	1	c''' loud
$c^{IV} : a^{IV} = 6826.6$	3 : 5	f''' audible.	2	1365.3	$> <$			
7168	4 : 7	g''' weaker.	3	1536	c'' .	768	3	g'' loud
$c^{IV} : b^{IV} = 7680$	8 : 15	Distinct.	682.6	1	f'' audi
7936	16 : 31	$> <$	512	1	c'' more
8064	32 : 63	Audible			ful tha
$c^{IV} : c^V = 8192$	1 : 2		256	...	c' loud.
						128	...	c loud.
						64	...	C perce
								beats.
								Octave

[To be continued.]

LII. On Salt Solutions and Attached Water.

By FREDERICK GUTHRIE.

[Continued from p. 369.]

Correlation between the Boiling and the Glaciation of Salt Solutions.

§ 139. **L**OOKING upon the phenomenon of solution as being brought about by the exercise of the force, physical affinity, and regarding the act itself as being in the first instance caused by the prevalence of what may be called physical adhesion (as distinguished from mechanical) between the water and the salt over the sum of the physical cohesions of the water and of the salt, we must after the first contact (that is, when some of the salt is dissolved) regard the contest as between the physical adhesion between the salt and the solution of it on the one hand, and on the other the physical cohesion of the salt together with the physical cohesion of the solution of it. By an increase of temperature the physical cohesion of the salt is lessened. By the solution of more salt the physical adhesion between the salt and the solution is lessened. Thus a partially saturated solution so far resembles what some chemists call a partially saturated atom, that it exhibits not necessarily a lesser

energy, but always a lesser capacity for saturation than a still less saturated one. (Witness anhydrous sulphate of magnesium compared with the crystalline salt.) Let us suppose that at a temperature t° a weight w of water dissolves $\frac{1}{n}$ of s , the weight of the salt. Then using the symbol \frown to denote either physical adhesion or cohesion, we have the condition of equilibrium or saturation expressed by the equation

$$s\left(1 - \frac{1}{n}\right) \frown \left(w + \frac{s}{n}\right) = s\left(1 - \frac{1}{n}\right) \frown s\left(1 - \frac{1}{n}\right) + \left(w + \frac{s}{n}\right) \frown \left(w + \frac{s}{n}\right).$$

Or, since the tensions of physical adhesion and cohesion are independent of quantity,

$$s \frown \left(w + \frac{s}{n}\right) = s \frown s + \left(w + \frac{s}{n}\right) \frown \left(w + \frac{s}{n}\right).$$

At a temperature t_1° ,

$$s \frown \left(w + \frac{s}{n+d}\right) = s \frown s + \left(w + \frac{s}{n+d}\right) \frown \left(w + \frac{s}{n+d}\right),$$

where d is $+$ or $-$ according generally as t_1° is $>$ or $<$ than t° .

The expression

$$s \frown \left(w + \frac{s}{n}\right) > s \frown s + \left(w + \frac{s}{n}\right) \frown \left(w + \frac{s}{n}\right)$$

implies that the water is not saturated ; while

$$s \frown \left(w + \frac{s}{n}\right) < s \frown s + \left(w + \frac{s}{n}\right) \frown \left(w + \frac{s}{n}\right)$$

implies that it is supersaturated. The above equations are the general ones for all salt solutions above 0° C. For temperatures below 0° C. another element manifests itself with greater force as the temperature is lowered, namely the physical affinity between the solid salt and solid water. If in fig. 1 we draw a straight line parallel to and below the zero axis so as to cut any one tracing in two points (it never can be cut in more than two points), we see that solidification may ensue at the same temperature in two differently strong aqueous solutions of the same salt. The intersection with the left-hand branch is the condition of equilibrium resulting from the equality between:—on the one hand, ice \frown ice together with salt \frown water ; on the other, salt solution \frown water,

$$\text{ice} \frown \text{ice} + s \frown w = \left(w + \frac{s}{n}\right) \frown w.$$

The intersection on the right-hand branch shows the condi-

tions under which there is equilibrium between:—on the one hand, salt \curvearrowright salt (or hydrate of salt \curvearrowright hydrate of salt) together with the salt solution \curvearrowright salt solution; on the other, salt solution \curvearrowright salt or hydrate,

$$\text{salt} \curvearrowright \text{salt} + \left(w + \frac{s}{n}\right) \curvearrowright \left(w + \frac{s}{n}\right) = \left(w + \frac{s}{n}\right) \curvearrowright s.$$

Looking upon the cryohydrate as a common value, we have

$$\text{ice} \curvearrowright \text{ice} + s \curvearrowright s + \text{ice} \curvearrowright s = \left(\frac{s}{n} + \frac{w}{m}\right) \curvearrowright w + \left(\frac{s}{n} + \frac{w}{m}\right) \curvearrowright s.$$

§ 140. Compare now the decomposition of a salt solution by the loss of heat with the decomposition by gain of heat when such a solution boils. And in instituting this comparison, we must bear in mind how much more sensitive to variation in pressure is the boiling- than the solidifying-point.

(1) A solution poorer than the cryohydrate loses heat; ice is formed.

(2) This goes on until the proportion of the cryohydrate is reached, the temperature falling.

(3) The cryohydrate may be reached by freezing out ice from a weaker solution, or by any other withdrawal of water.

(4) When ice separates from a liquid, it remains in contact with the liquid, and endeavours to redissolve therein.

(5) When by the separation of ice the proportion of the cryohydrate is reached (nearly independent of pressure), ice and the salt separate simultaneously.

(6) The two bodies (ice and the salt) being crystallizable solids, unite to form a crystallizable cryohydrate which exhibits a constant gravimetric composition.

(7) A cryohydrate in the act of solidification shows identity of composition between the solid and liquid portions. The temperature of solidification is constant.

(1) A solution poorer than that saturated at a given temperature receives heat; vapour is formed.

(2) This goes on until saturation is reached, the temperature rising.

(3) Saturation may be reached by evaporation, boiling, or any other withdrawal of water.

(4) Vapour separated from a liquid is removed from the field of contention, unless the liquid be enclosed with the vapour.

(5) When by the separation of vapour the proportion of saturation is reached (very dependent upon pressure), vapour and the salt separate simultaneously.

(6) One being a solid and the other a vapour, they do not unite, but in their separation preserve a constant gravimetric ratio under like conditions of pressure.

(7) A saturated solution, when boiling, shows the same ratio between the vapour formed and the salt precipitated as exists between the liquid water present and the salt it holds in solution. The temperature of boiling is (under like pressure) constant.

§ 141. A cryohydrate undergoing solidification may therefore be considered physically as the homologue of a saturated salt solution in the act of boiling. For absolute comparison we should have to eliminate pressure, or rather make automatic pressure the measure of the effect of elimination. Wüllner (*Pogg. Ann.* Bd. cx.) found that the vapour-tension of a salt solution varied inversely with the amount of salt it contained. Thus at $51^{\circ}8$ the vapour-tension of aqueous solutions of chloride of sodium, in mercury pressure, were found to be:—

H ₂ O.	NaCl.	millim.
100	0	100
90	10	94
80	20	88

Wüllner also found that the diminution of the vapour-tension effected by a given percentage of the salt increases as the vapour-tension increases; that is, the diminution is greater as the temperature is greater. Looking upon vapour-tension as the mechanical equivalent of effort at separation, we find that it has its perfect counterpart in the left-hand branches of the curves of fig. 1. Compare the quantity of vapour with the quantity of ice. The left-hand branches are nearly straight lines; but they are on the whole clearly concave downwards. The variation in the nature of the salt also affects the vapour-tensions; and on constructing the curves of vapour-tension of different salts in solutions of different strengths, we see that the ice-curve is algebraically continuous with the vapour-tension curve. The same may be also seen if we assume that the atmospheric pressure, being constant, will not affect the order of curvature of the curves representing the ratio of salt in a solution and its boiling-point.

§ 142. The fact that the separate constituents of a solution of the proportion of a cryohydrate are both of one form of matter, of course favours their combination. With boiling aqueous solutions the dissimilarity of the educts is generally unfavourable to their union. There is ample evidence, however, of a similar, though less definite (because more influenced by pressure), reunion of the constituents of certain solutions. If an indefinite quantity of dilute alcohol be rectified 100 times at a constant pressure in such a manner that the first (say half) portion is redistilled, the water is never eliminated, nor does the strengthening extend beyond a definite limit. Perhaps the experiments bearing most directly upon this train of thought are those of Roscoe and Dittmar on the hydration of acids. Since, for instance, nitric acid containing 80 per cent. of HNO₃ is weakened on boiling (consider-

ing the residual body), and an acid containing 60 per cent. is strengthened by boiling, until in both cases an acid containing nearly 70 per cent. of HNO_3 is got, which boils at 120°C. at 760 millims., this body was viewed as a definite hydrate. Roscoe showed that this acid, if it be boiled at a lower or higher pressure, is itself decomposed. So with hydrobromic, hydrochloric, and hydriodic acids, with sulphuric acid, and with ammonia. We have here physical ratios conditioned by pressure. Without doubt the cryohydrates would vary in composition if they were formed at enormous pressures; for the variation effected by pressure in the freezing-point of water is not likely to be precisely the same as the variation in the solidifying of a salt out of a solution. Trifling as these differences would assuredly be under all conditions of variation of artificial pressure, the thought is not remote that we may have to refer the extraordinary variation in quantity of the elements of some perfectly well crystallized and constantly composed minerals from what is sometimes called the atomic ratio to the enormous pressure attending their genesis. The fact that such composition may be represented on paper by the complex manipulation of ratios known amongst laboratory products, can only be admitted into the argument when their formation can be shown in the laboratory by means such as may be fairly supposed to exist in nature. In a word, under vast changes of temperature and pressure, the gravimetric saturating quantities, or "atomic weights," of elementary substances may vary. And it is perhaps noteworthy that this conception, which has urged itself upon us with rare force in the consideration of the relations by weight of bodies in union at extremely low temperatures, is the same as that which has been brought forward by some of those who have examined the relation of the elements when subjected to the high temperature and abnormal pressure of the sun's mass, which has, as a doctrine of continuity, commended itself to some of the most philosophic chemists, and which is certainly in harmony with the conception of dynamic energy, whether of mass or mere velocity.

Our chemistry, as it is generally understood, is the chemistry of one atmosphere pressure.

Separation of Ice from Solutions of Mixtures of Salts

§ 143. In III. §§ 109–122, I proved that on cooling a saturated solution of two salts, the temperature of the cryohydrate, when reached after separation of various quantities of the constituents, was never sensibly lower than that of the

constituent which had the lowest of the two cryohydrate temperatures. In these experiments I was careful to use the salts in such proportion that recombination in integral quantity was possible. In the series of experiments now to be described, where ice has to be separated from a mixture while the salts themselves remain in solution, we may, in those cases at all events where no double decomposition is possible, take equal weights of the salts, so as to compare the results with those of fig. 1; and for this study I will here confine myself principally to the two nitrates AgNO_3 and NH_4NO_3 , because nitrates are especially free from tendency to form double salts, and because the tracings of the ice-separating temperatures of the individual salts are of widely different range and character.

§ 144. *Separation of Ice from mixed Solutions of the Nitrates of Silver and Ammonium.*—The following are all 20 per cent. solid.

Sol. 1.	AgNO_3	. .	20 grms.	}	gave ice at -4.3 .
	NH_4NO_3	. .	10 "		
	H_2O	. . .	120 "		
Sol. 2.	AgNO_3	. .	20 "	}	" " -5.0 .
	NH_4NO_3	. .	20 "		
	H_2O	. . .	160 "		
Sol. 3.	AgNO_3	. .	20 "	}	" " -5.5 .
	NH_4NO_3	. .	30 "		
	H_2O	. . .	200 "		
Sol. 4.	AgNO_3	. .	10 "	}	" " -5.8 .
	NH_4NO_3	. .	20 "		
	H_2O	. . .	120 "		

The following are all 30 per cent. solid :—

Sol. 5.	AgNO_3	. .	30 grms.	}	gave ice at -6.8 .
	NH_4NO_3	. .	15 "		
	H_2O	. . .	105 "		
Sol. 6.	AgNO_3	. .	30 "	}	" " -8.0 .
	NH_4NO_3	. .	30 "		
	H_2O	. . .	140 "		
Sol. 7.	AgNO_3	. .	20 "	}	" " -8.9 .
	NH_4NO_3	. .	30 "		
	H_2O	. . .	116.6 "		
Sol. 8.	AgNO_3	. .	20 "	}	" " -9.1 .
	NH_4NO_3	. .	40 "		
	H_2O	. . .	140 "		

On comparing these results with those given in fig. 1 (showing the ice-tracings for the separate salts, which are also

20 and 30 per cent. solid), the following results are obtained in regard to the comparative influences of nitrate of silver and nitrate of ammonium in retaining ice in solution.

TABLE XXVII.

Solution.		Calculated.	Found.
20 per cent.	1. $\text{AgNO}_3 = 2 \text{ NH}_4\text{NO}_3$	-4.1°C.	-4.3°C.
	2. " = 1 "	-4.8	-5.0
	3. " = $\frac{2}{3}$ "	-5.3	-5.5
	4. " = $\frac{1}{2}$ "	-5.5	-5.8
	5. " = 2 "	-6.9	-6.8
30 per cent.	6. " = 1 "	-8.1	-8.0
	7. " = $\frac{2}{3}$ "	-8.8	-8.9
	8. " = $\frac{1}{2}$ "	-9.2	-9.1

The calculated numbers are got under the hypothesis that the influences of the constituents of the mixture are proportional to their masses. That is, if there be n grms. of A and m grms. of B, each being a p per cent. solution, and if A alone gives out ice at t_1 and B at t_2 , then (t being the temperature at which the mixture gives up ice)

$$t = t_1 + \frac{n}{n+m} (t_2 - t_1);$$

and the numbers found do not differ from those calculated more than may be attributed to errors of observation.

§ 145. *Separation of Ice from the mixed Sulphate and Nitrate of Ammonium.*—Of the 10-per-cent. solution of the sulphate of ammonium 20 grms. were taken; of the 10-per-cent. solution of the nitrate of ammonium 10 grms. were taken. The mixture of these gave up ice at -3.2° , which is exactly the temperature calculated from the above equation, the glaciating points of the two solutions being -2.6° and -3.5° respectively. We may assume therefore, generally, that the temperature of glaciation of a mixture of two salt solutions of the same percentage strength is a mean between the glaciation-temperatures of its constituents, when new salts are not formed. This chapter, however, will require a great amount of further work, especially for those cases where the constituent solutions of the mixture are not of the same strength.

§ 146. *Further examination of Iodide of Sodium.*—In conclusion I am very glad to be able to throw a clear light on the anomalous and exceptional behaviour of the iodide of sodium. From §§ 65, 68, 69, it appears that while as a cryogen this salt gives the temperature -26.5° , it solidifies as a cryohydrate at -15° . A large quantity of the iodide,

purified by several recrystallizations, was dissolved to saturation at 12° C., and very gradually cooled to 0° . The crystals given up in this range proved to be anhydrous iodide. On further cooling to -12° , a second crop of crystals was formed, which also proved anhydrous. Between -12° and -20° but little further solidification occurred. When -23° was reached rapid solidification began, and the temperature rose to -15° . In another similar case the temperature sank to -24° before solidification began. The whole becomes of a dough-like pastiness. The crystals are long, silky needles; they remain transparent. On further cooling in a chloride-of-calcium cryogen to -26° , the silvery opacity due to a true cryohydrate is produced, and the whole becomes of a stony hardness. It is clear, therefore, that with the iodide as with the chloride of sodium there are two cryohydrates:—the subcryohydrate, having a melting- and solidifying-point at -15° ; and another, which in combination with the first has a melting- and solidifying-point at $-26^{\circ}5$. We have therefore to look for the true (that is, lowest-temperated) cryohydrate in solutions weaker than that analyzed in § 65, which had 59.45 per cent. With the chloride of sodium there is no difficulty in separating the subcryohydrate; the latter body falls as iridescent scales, and leaves the residual liquid clear. With the iodide of sodium no such mechanical separation ensues, and no artificial separation demanding the maintenance of so low a temperature would be trustworthy.

To get at the composition of the true cryohydrate it must be approached from the other side—not from saturated, but from dilute solutions (see § 123). I have accordingly examined in succession the temperatures at which ice or other body separates from *percentage* solutions of NaI.

TABLE XXVIII.

NaI per cent., by weight.	H ₂ O per cent., by weight.	Temperature C. at which solidifi- cation begins.	Nature of solid separated.
5	95	— 0.7	Ice.
10	90	— 2.1	"
15	85	— 3.9	"
20	80	— 6.0	"
25	75	— 8.5	"
30	70	— 11.8	"
35	65	— 15.2	"
40	60	— 20.5	"
45	55	— 26.0	"
49.2	50.8	— 30.0	Cryohydrate.
50	50	— 29.5	Subcryohydrate.
55	45	— 20.0	
60	40	— 14.7	
61.6	38.4	0.0	NaI.
63.6	36.4	+ 13	"

With regard to the first eight determinations there is nothing particular to remark. For the 40-per-cent. solution a CaCl_2 cryogen had to be used. Finding that ice separated from the 45 solution, and the subcryohydrate from the 50 solution, I concluded that the composition of the true cryohydrate must lie between these values. I accordingly submitted several ounces of the 45, and also of the 50 solution to a carbonic-acid cryogen. From the first ice separated; from the second the subcryohydrate. The temperature sank in both cases to -30° , and there remained constant, while from both crop after crop of the pearly-white true cryohydrate was formed. After about three quarters of each had been thus removed, the temperature still remaining at -30° , the residual liquids were analyzed by evaporation and strong heating.

Of 45 solution 10.9945 gave 5.3982 NaI or 49.1 per cent.

Of 60 " 14.6176 " 7.1932 " " 49.2 "

Accordingly the true cryohydrate is reached from either side. The water worth of this salt is 8.603. It would only be possible to deduce the composition of the subcryohydrate if we knew the relative quantities of the two present, as well as the composition of their compounds. The reason why the dry iodide does not give the temperature of -30° when used as a cryogen is no doubt because, like chloride of calcium, it evolves a large amount of heat in coming into contact with water. But in spite of this deficit, the temperature of the cryogen is governed, as we have seen it to be before, by that of its coldest constituent. The speculation concerning iodide of sodium in § 69* is fully justified.

For the solubility at 0° , a solution saturated at 8° was kept for three hours at 0°C . Of this, 10.7932 grms. contained 6.6486 grms. of this salt: this gives 61.6 per cent.; Kremers found 61.3.

§ 147. No salt can well be more favourable than the iodide of sodium for the study of double supersaturation. The values of the solution between 49.2 and 61.6 show this phenomenon in a remarkable manner. Thus a solution of 60 per cent. may be kept for hours at -20° . It may be stirred, shaken, particles of dust and particles of NaI may be thrown in, a glass rod covered with a film of hoar-frost inserted, it remains quite limpid. Cooled a few degrees lower, it becomes suddenly semisolid, and rises to -15° or -14° in temperature. At values approaching the cryohydrate, but rather less strong, it may happen, as the excess of water is removed by cold, that instead of the cryohydrate being formed when the right

* In this paragraph read "cryohydrate of iodide of sodium" instead of "cryohydrate of sodium."

strength is reached, the suberyohydrate is produced continuously with the further separation of ice. The one set of crystals floats, the other sinks. This occasional state of things is the most perfect counterpart of the simultaneous separation of water as vapour and the salt when a saturated solution boils, considered in § 140. A drop or two of the same solution, suddenly cooled to opaque solidification, determines the formation of the true cryohydrate.

I beg the reader to note the following corrections of, and additions to, Table X. § 88. In the case of correction, the corrected number is marked with *, the previously given number being placed in brackets.

Formula of salt.	Temperature of cryogen.	Temperature of solidification of cryohydrate.	Water-worth.	Per cent. of salt.
Add $\text{CaCl}_2 + 3\text{H}_2\text{O}$	—33	—37	11·8	36·45
Correct... NaI	—26·5	—30* (—15)	8·6	49·2
Correct... NH_4Cl	—16	—16* (—15)	12·4	19·27
Add AgNO_3	— 6·5	— 6·5	10·09	48·38
Correct... KNO_3	— 3	— 3* (—2·6)	44·6	11·20

LIII. *Points in a Programme of Physics.* By DON ENRIQUE SERRANO Y FATIGATI, *Professor in the Instituto Nacional, Ciudad Real.* Abstracted and communicated by EDMUND J. MILLS, *D.Sc., F.R.S.**

Introduction.

THE recent history of science is one of accumulations, bedimmed by detail. As we survey it, we observe a twofold confusion—that arising from the admixture of innumerable single items, and that which is due to the interference of imagination. Thus even its facts have become tintured with a kind of unreality. And, while unquestionably admitting that without observations and quantitative measurements we cannot acquire an adequate knowledge of phenomena, it must equally be allowed that without philosophical investigation we must constantly wander in an inextricable labyrinth of facts. But a part of the confusion is traceable to our taking into account the Being, Form, Essence, Causality, &c. of things; and these cannot possibly be reduced to experiment.

* From the *Revista de la Universidad de Madrid*, (2) v. 61. A further development of the author's views, of more especial interest to the mathematical reader, is given in *Una Leccion de Fisica general* (Madrid: Aribau, 1876, pp. 45).—I wish to record my indebtedness to Mr. C. Tomlinson, F.R.S., who very kindly wrote me a translation of the whole of the difficult memoir here represented in abstract.—E. J. M.

Consider, for example, the case of physics. Can we unequivocally say that it clearly knows its own object? Does it hold within exact and rigorous limits its own definition (which occurs in every treatise) as "the science which studies the phenomena of bodies whose intimate composition remains unchanged"? The relations of composition to other properties are, indeed, but imperfectly known; and even the light of the undulatory theory becomes fainter when we arrive at the focus of the phenomena to be interpreted. What, again, of the æther—that subtle substance which, in an immense vacuum, transmits unaltered from point to point the disturbances of its infinite material? In a small group of memoirs, such as those of Plücker on Diamagnetism (further developed by Edlund), the laws regulating the equilibrium of fluids have indeed been somewhat applied to it; but our conception of its nature remains most incomplete. Yet undulations are real, and the interferences of undulations are real. They constitute the ascertained basis of the mechanical theory of heat—a theory so well founded and fruitful, that it ought to induce us to verify our beliefs in other scientific departments.

The common practice of formally representing to our imagination as atoms the individual elements of bodies, without inquiring whether the proceeding is false or sound, has led to a vague conception of mathematical points, or centres of force, whose activity is only virtual. This has involved a clear loss of predictive power. Such a conception, also, pulverizes natural unity in order to reconstitute it afterwards. Nor does it reconstitute a harmonious unity, such as an extraneous and dissevered net, with knots corresponding to each material atom. That the atomic doctrine gives a very obscure presentation of subjects of the greatest importance, will not be denied by its most passionate defenders; that it delivers no rational or exact account of actions at a distance, is also quite evident; that the laws of definite and multiple proportions, in which the doctrine has in a manner cemented itself, may be interpreted otherwise, is an affirmation of which any one may most easily convince himself. Now, from the darkness which surrounds the problem of the constitution of matter, we can only emerge by having recourse to first principles. And since in the impenetrable and extensible atom we seem to be almost the victims of a fresh phlogiston, and both nomenclature and hypothesis have become infiltrated with atomism, the dismissal of the atomic theory must imperatively be demanded. Doubtless the more it advances, the more it is beset with innumerable difficulties; and at each stage of the process who does not ask for something better than those micro-microscopic stellar

systems which, in the bulk of a cubic millimetre, are mimics of a million nebulæ?

But it was impossible that the state of these sciences should be otherwise. Every student of physics or chemistry is continually encountering such terms as *force*, *matter*, *movement*, and the like—terms which are ill-defined and are the bases of alleged important theories, terms in a series of which no one knows which is the ultimate operator. Other sciences have been subjects of a similar caprice, and render more vivid the confusion. In unity of plan, and in appropriating the great results of kindred sciences, all have been deficient.

If science is ever to offer an exact reflection of the object of which it is the image, it will do so by appreciating the clear and definite proof that it is dealing with an essential and indivisible unity, not a unity produced by the mere heaping-up of details.

The study of physics requires five necessary considerations. These are:—the conception of physics; its plan; its relations with other branches of knowledge and with human life; its sources of knowledge, and mode of investigation. The unity of such a branch will appear well marked on such a basis, and in sympathy with the aspirations of all its cultivators. We exclude, on the one hand, the unscientific and dreamy metaphysics which Newton stigmatized; we admit, on the other, the legitimate philosophy which deals with palpable and rational foundations.

Idea of Physics.

I. *Energy*.—Physics, if broadly considered, is identical with nature; but such topics as physiology, morphology, natural history, and the like are not generally included in it. In nature we observe a multitude of objects and forces:—man, the stars, animals, plants; heat, light, electricity, chemistry, gravitation; and a creative force, modified by all the other forces. Between all these, relations exist which cannot be manifested in one set apart from the others; there is a harmony, demonstrative of order, even among radical differences. Man himself is not simple but twofold, containing the infinitely developable elements of mind and body. Nature is a total, complete, and necessary existence; its union with mind forms the finite series of the universe, whose ultimate term is a creative being. The innate force of nature, her essence, form, and mere existence, are but vague abstractions. We may think such abstractions—think them as independent of the series in which they occur (mind apart from body, for instance); but they have no place in the realm of integers and facts.

Natural existence, being subordinate to absolute unity, presents two distinct modifications : one is existence out of time, and so far eternal ; the other, the temporal and transitory. The union of these two constitutes *life*, which is the manifestation of the infinite in time. Nature in her pure unity is indeterminate, simply indicating potentiality. But she undergoes a *nisus* after development, and then presents us with infinitely varied modes. These three logical moments are realized at one and the same time, nature being simultaneously and eternally one, varied, and entire ; the process of this evolution is the process of life. The realization of such a process requires an essential unity—an infinite variety of states, and an intimate dependence and relation between the units and their sum. Thus, while each individual has a separate existence, the whole is not made up by mere juxtaposition of parts. This idea of tri-momental evolution agrees with actual observation ; we shall find it in the birth, process, and extinction of any genus, species, or individual, whether we refer to ordinary living beings or the more distant substance of the stars. In the midst of all these changes, whether of person or of system, each man recognizes his own permanent and continuous unity, and thence observes the systemic unity.

Each individual—whether star, plant, animal, or man—is modelled on the infinite variety of nature, as well as on her unity. Whithersoever we look—be it to the sun for heat and light, or to any of the celestial bodies for what we term gravitation—we meet with forces derived from individual functions of various kinds. Ascending higher, we see clearly that such forces are not distinct, and independent of the one activity of nature, that their ordinary laws are but laws of mode with which she invests herself, and that heat, light, gravitation, and the like are general determinations in which she manifests herself. But the sum total of energy in nature remains one and the same.

The evolution of life and the evolution of natural forces are thus strictly analogous. Whichever be the field surveyed, we always come at last to the activity of nature, in infinitely seriated motion. The branch of human knowledge which studies that activity is the science of energy.

Energy thus refers to natural activity, whether potential or real. Among the problems connected with it may be mentioned, Whether the total activity existing in any one system of bodies, or in all nature, remains constant ; Under what conditions it may occur—as in gravitation, heat, light, etc. ; What are the metamorphoses due to such and such conditions of the organic process ; and, *à posteriori*, How to establish

and determine the logical value of the abstract unity sought by physicists.

But physics, as at present studied, has reference solely to the general forces or agencies of nature. We must add to this a higher and more philosophical regard. The science of energy treats of an activity which is in itself immutable, and in the unity of its object belongs to nothing that is partial or transitory.

II. *The Idea of Energy as compared with the actual state of Physics.*—The idea of energy agrees with our idea of physics, including the fundamental parts of chemistry and mechanics. Not unfrequently in the recent history of physics have we seen that science ascertaining the relations between individual facts or forces, and, these transcending, rise to their approximate source. Mechanics and chemistry have sometimes manifested strict relations, despite their independence and difference of method and object. The former being the general doctrine of motion, and the modelling under which energy displays itself, has to determine step by step the different positions of activity; hence its customary separation from the latter is scarcely real or effective. Chemistry certainly shows a very different aspect from that of physics, as a history differs from a philosophy; yet when we enter the field of chemical affinity, we must admit that to be part of the domain of energy.

The revolution which has taken place in modern physics has been distinctly in favour of unification, and would never have been accepted without a tacit faith in the new ruler. The researches of Fleming on the contact theory of electricity, and of Becquerel on electro-capillary phenomena, which at first seemed to controvert the splendid labours of Faraday, have since been harmonized therewith; but, in the verifying process, we have been compelled to take into account the bearing of thermodynamics and mechanics on the various results. Crookes has brought into prominence, in connexion with another order of forces, the intimate connexion between gravitation, light, and heat. Volta has shown the relation of heat to the electromotive forces of differing couples. Holzmänn has determined the dielectric constants of insulators. Merget has succeeded in reproducing with powders and pulverulent bodies the phenomena of thermo-diffusion exhibited by vegetable organisms. Edlund's theory of electricity has thrown a most vivid light on all that was obscure in that branch of knowledge. Thermodynamics has lent aid to a vast number of questions not pertaining simply to heat.

What physics represents at the present time, may be best observed by ascertaining some of its precedents, and following

them down to our own time. Thus, Huyghens inaugurated the modern theory of light with his fruitful theory of waves, whereby we can foretell in an admirable manner the mechanism of propagation. *Foretell*, it must be said, rather than *demonstrate*; for the undulatory theory is tinged with abstraction, involves more of geometry than of physics; and we at present limit our attention too much to the individual wave, which is destitute of that interpenetration and mutual influence which reality requires. The periodicity observed by Newton in coloured rings, and his analysis of light by dispersion, at first seemed contrary to Huyghens's theory, but have since proved to be its harmonious support. Behind the gigantic figure of Newton we observe Euler and Young, each of whom fixed his mark on the embryonic evolution of modern thought. The former established analogies between colour and sound, and showed that both depend on undulatory periods; the latter established experimentally the existence of interferences. Fresnel made yet further progress by tracing the modifications which light undergoes in its transit through crystals, thus assisting us to form some conception of the intimate structure of bodies.

In consequence of these investigations, and the results by which they have been followed, we now have recourse to energy without troubling ourselves theoretically as to its particular form; we can measure each result in accordance with the principle of unification. Such a possession is all the more noble when we bear in mind the colossal nature of the forces to be dealt with.

We seek, and we find, relations between all the forces now known to us, and we discover that they all spring from, and are contained in, one natural energy. Can the conditions and the time be more favourable for advancing a step further in this path, and tracing in all its fulness the solid unity of the constitution of physics? No other science is so well prepared for this reform; at least, no other indicates it.

We see, lastly, in this movement that which, in my poor judgment, raises it to a much higher and more splendid position. We see in it forces sufficiently energetic to be independent of hypotheses; we hear of researches in which, at each step, the word *atom* needs no longer be pronounced. To sum up briefly, we may affirm that the conception of energy here adduced is in perfect harmony with modern physics; but chemistry and mechanics have to be submitted to its sway. The actual condition of the science of energy is a supreme and instinctive aspiration after unity—not an abstract and vague unity, but one based on determinations and details. How we

should retrace our steps from this practical and partly realized ideal, and revert to those lesser paths in which the science of physics should be developed, must be discussed in a future communication.

LIV. *On the Induction Spark produced in breaking a Galvanic Circuit between the Poles of a Magnet.* By B. O. PEIRCE, Jun.*

BECQUEREL, in the *Journal de Physique*, vol. iv. p. 206, states that the spark obtained by breaking the current which circulates through an electromagnet, between its poles, is entirely a mechanical effect.

To test the accuracy of M. Becquerel's result, I placed in the same circuit four Grove cells, an electromagnet of the form used in experiments upon diamagnetism, and a coil of coarse wire wound round a bundle of iron wires on a core. Over this coil was slipped an induction-coil of 6000 ohms resistance.

I first experimented upon the spark given by the induction-coil when the primary circuit was broken between the poles of the electromagnet, and then outside of them.

The number of cells in the circuit was varied, and its resistance was several times changed, in order to vary the circumstances as much as possible; and at each change a series of observations were taken to see whether the spark from the induction-coil had any greater power of overcoming resistance when the primary circuit was broken between the poles of the magnet.

A long series of observations on sparks, which were of all lengths from $2\frac{1}{2}$ to 15 millims., justifies the statement that the distance over which the spark of the induction-coil would regularly leap was more than *doubled* when the circuit was broken between the poles. The absolute length of spark obtained depended, of course, on the electromotive force in the primary circuit being apparently, within certain limits, directly proportional to it. In almost all cases the length of spark was increased to three or four times; and in no one set of observations was the spark less than doubled in length. These experiments were repeated several times on different days.

I found great difficulty in breaking the primary circuit uniformly; and it was only after a long time that my results became regular.

I tried breaking from the surface of mercury; but although the same relative effects were attained, the actual effect was far

* Communicated by the Author, having been read at the American Academy of Arts and Sciences, February 9, 1876.

inferior to that obtained when two bright copper wires were separated in the air.

Rowland has shown that a very powerful electromagnet is not needed in order to obtain good results in studying diamagnetism. In order to see whether a small magnet would not do to increase the length of the spark, a small electromagnet, capable of supporting perhaps two kilogrammes, was set up, and a series of observations taken when the circuit was broken between its poles. In this case the length of the spark was just doubled. Might not a rather small horseshoe electromagnet be advantageously placed upon the Ruhmkorff coil so that the primary circuit should be interrupted between its poles?

I next put the electromagnet in one circuit, and broke another circuit containing the primary of the induction-coil between the poles of the magnet. With this arrangement two sets of observations were taken under slightly different circumstances. The results are given below, in the actual space over which it was found that the sparks would just pass. The words "outside" and "inside" denote that the primary circuit was broken outside of the poles and between them respectively.

Outside.	Inside.
$2\frac{1}{2}$ millims.	$12\frac{1}{2}$ millims.
3 ,,	20 ,,

Of course the relative increase in the length of the sparks depended upon the relative strengths of the two currents. When the electromagnet is very weak and the outside current very strong, subsequent experiments showed that there was little, if any, increase in the length of the spark.

As a direct means of showing that the extra effect obtained by breaking between the poles is not mechanical, the following method was employed. The electromagnet was placed in a circuit with two Grove cells. To the end of one of the large wires used for breaking the circuit a very fine wire was fastened, leading to one pole of a Thomson's quadrant electrometer, whose other pole was put to earth. Whenever the two large wires were separated, there was of course a deflection of the electrometer-mirror, corresponding to the difference of potential of the poles of the battery; but beside this there was an increased effect when the circuit was broken between the poles, that seems to be unaccountable if we assume the effect to be mechanical.

When a condenser of about 1 farad capacity was put into the circuit the following deflections were obtained :—

Primary broken outside.	Primary broken inside.
21	27
20.5	28
22	28
20	27
Mean 20.9	27.5

When no condenser was used the deflections were as follows:—

Primary broken outside.	Primary broken inside.
20	22.5
19	23.5
20	23.5
20	24
19.75	23.4

The electromotive force in the circuit was very small, hardly enough to render the poles strongly magnetic. With a proper electromotive force we might expect the effects to be more striking.

In order to measure the increase of quantity obtained by breaking the primary circuit between the poles of the magnet, the electromagnet and the primary of an induction-coil were placed in the same circuit as at first. The poles of the induction-coil were connected (the one directly, the other by means of the spark leaping between two points) with a condenser of $\frac{1}{3}$ farad capacity. The poles of the condenser were connected with a Thomson's galvanometer of 5880 ohms resistance. Each of the following results is the mean of two observations. The length of spark was the same in both cases.

	Primary broken outside.	Primary broken inside.
6 cells.	17.5	21.05
4 „	12	17

The resistance of the six cells (Bunsen) was greater than the resistance of the primary circuit.

On another day the same arrangement gave the following result, where each is a mean of two observations:—

Length of spark.	Primary broken outside.	Primary broken inside.
1 millim.	22	30
3 millims.	12	23

In a great many cases, where the magnet was quite powerful, it was not easy to get comparable results. When the primary circuit was broken between the poles, the spot of light was driven off the scale; but when the primary was broken outside, there was frequently very little deflection.

At another time a Thomson's mirror-galvanometer of 6 ohms resistance gave, with an arrangement otherwise the same as the above, the following deflections as a mean of twelve observations:—

Outside ... 12·9 Inside ... 15·37

When the electromagnet was in a separate circuit from the primary of the induction-coil, the following observations were obtained with the Thomson's galvanometer. Each result is the mean of about twenty observations.

Length of spark.	Primary broken outside.	Primary broken inside.
1 millim.	21	35·8
2 millims.	20	23
3 "	16·5	19·9
4 "	15·2	17

All these results were taken with interrupting the primary circuit by separating two copper wires held one in either hand. Practice made the results then obtained quite accurate. I at first tried breaking from the surface of mercury; but, beside the irregularity in the amount of the deflection, there was a most unaccountable change of polarity every few moments. I distrusted the evidence of my own senses so much in this case that I asked several other people to observe for me, without previously telling them any thing of this change of polarity. In each case the observer noticed the reversal for himself. The deflection was almost always in one direction when the circuit was broken between the poles, and in the other direction when the circuit was broken outside of the poles. This rule is not, however, absolute. This reversal of polarity only occurred when *copper* was used to break from the surface of the mercury. The separation of iron from iron, or copper from copper, or iron from mercury, never gave any reversal. The reversal was best seen with the electrometer.

At several different times I took a series of observations upon the deflection, by breaking inside the poles and outside of them, when the distance over which the spark had to leap was varied. The curves obtained by laying down these deflections were not, as one might suppose, hyperbolas, but were apparently exponential curves, having the axis of x as an asymptote, but not the axis of y . A series of very careful observations was taken by observing the deflections when the distances over which the spark passed were small. The curves obtained by breaking the primary between the poles were similar to those obtained when the primary was broken outside. The observations were taken by breaking the primary with the interrupter of one of Ritchie's induction-coils. The sparks passed between two circular disks of copper 10 centims. in diameter. In the centre of one of the disks was an almost imperceptible protuberance, in order to ensure the sparks always passing in the same place. One plate was fixed in a horizontal position; the other was suspended, by a thin ivory handle perpendicular to its plane, to a glass rod placed in the telescope-socket of a cathetometer. In this way it was found possible to get the plates sensibly parallel. The poles of the induction-coil were connected with a condenser—one directly, and the other by means of the spark passing between the two disks. The condenser was then discharged through a galvanometer. The micrometer-screw of the cathetometer reads easily to the $\frac{1}{1000}$ of a millimetre; and observations were taken with its aid at intervals of .050 millim. The plates were considered to be in contact whenever *making* the primary circuit gave any deflection in the galvanometer. The zero thus obtained was quite constant, whereas it was almost impossible to tell by the eye just at what point the spark ceased to pass when the circuit was broken. The poles of the battery were kept apart when not actually in use; and it was supposed that the electromotive force remained constant during the time of observation.

In laying out a curve, it must be remembered that there was a resistance of 6000 ohms already in the circuit. Each of the following results is the mean of a series of closely agreeing observations:—

Separation of plates.	Deflections.	Separation of plates.	Deflections.
millim.		millim.	
·050	183	·800	91
·100	176	·850	95
·150	164	·900	96
·200	145	·950	88
·250	130	1·000	93
·300	136	1·050	79
·350	123	1·100	85
·400	127	1·150	84
·450	120	1·200	70
·500	106	1·250	75
·550	102	1·500	63
·600	97	1·750	51
·650	92	2·000	50
·700	88	2·250	45
·750	90	2·500	42

When the sparks passed between the ends of two copper wires $\frac{2}{3}$ millim. in diameter, carefully filed so as to be parallel, the curves obtained were very regular, but of the same general shape. As an example I give the following :—

Distance over which the spark leaped.	Deflections.
millim.	
1	90
2	70
3	55
4	44
5	35·6
6	27
7	20
8	13
9	7
10	2·5
11	1·4
12	·8

Sir William Thomson has shown, in his paper "On the Electromotive Force necessary to produce a Spark," that a greater force per unit of length is needed for short distances than for long distances. He does not state in his paper whether he experimented upon the Ruhmkorff coil or the Holtz machine. In using the quadrant electrometer in measuring the electromotive force of the sparks from an induction-coil, it is, of course, necessary to use a small leaping-distance for the sparks, to avoid the return current. At times I have

found that a greater actual deflection was obtained when the leaping-distance was as great as $\frac{1}{2}$ millim. than when it was much smaller. May not Sir William Thomson's results be partly accounted for by induction in the same manner?

Another method of experimenting upon the extra spark obtained by breaking the circuit between the poles of an electro-magnet gave excellent results. One of the poles of the induction-coil was connected with the outer coating of a very small Leyden jar; while the other pole was connected with the inside coating through a small interval of air, to avoid the return current. The inside coating of the jar was connected by a very fine wire to a thin copper disk 261 millims. in diameter. Opposed to the copper disk, at a perpendicular distance of 160 millims., was the end of a short rod 1 millim. in diameter. Attached to the other end of the rod was a very fine wire connecting with one pole of the quadrant electrometer. The other pole of the electrometer was connected with the ground. The very fine wire leading from the opposing section of the rod was so arranged that experiment showed no inductive effect from the disk upon it. When the primary circuit was broken, a spark passed, charging the Leyden jar, and consequently the circular plate. The insulated plate was consequently charged to some constant potential V_0 .

According to Maxwell's 'Electricity,' vol. i. § 177, and Thomson's 'Papers,' 233, the surface-density at any point on a thin circular insulated plate is

$$\sigma = \frac{V_0}{2\pi^2 \sqrt{a^2 - m^2}},$$

where a^2 is the radius of the plate, and m the distance of the point from the centre.

If the plate is in the coordinate plane xy , we have

$$\sigma = \frac{V_0}{2\pi^2 \sqrt{a^2 - x^2 - y^2}}.$$

The potential at any point (x, y, z) in space due to this distribution is $\iint \frac{dy dy \sigma}{r}$, since the plate is thin. The limits must be so chosen as to comprehend the whole surface of the disk; and, to avoid errors, the point (x, y, z) must be opposed to the disk.

$$r^2 = (x - x_1)^2 + (y - y_1)^2 + z^2,$$

$$V_{x,y,z} = \iint \frac{V_0}{2\pi^2} \cdot \frac{1}{r} \frac{dx dy}{\sqrt{a^2 - x_1^2 - y_1^2}}.$$

At any fixed point (x_1, y_1, z_1) , therefore, the potential is pro-

portional to V_0 . It was supposed necessary that the potential of the quadrants attached to the short rod, which was at a great distance from the electrometer, should be proportional to V_0 . The opposite quadrants were at potential zero, being connected with the earth; and since when the deflections are small they are proportional to the difference of potential of the two poles of the instrument, it was supposed that the deflection of the electrometer-needle would be a relative measure of the potential of the plate.

A great many observations were taken with this apparatus; and the results agreed with the former ones, not only qualitatively, but very nearly quantitatively. I select the following series of observations to show this. The difference is in this case not so widely apparent, owing to the extreme weakness of the current used—it being at this time, in order to get small deflections, the weakest used in the whole of my work.

Primary broken outside.	Primary broken inside.
7.5	9.5
10	9
7.3	9.4
7	9.5
8.2	8.5
8.2	9.2
10.6	9.5
9.5	8.6
5.2	8.4
10	9.7
8.8	8.5
8	9.4
6.5	10.5
9.4	10.9
7	8.1
9.2	10
5.3	8.7
10.5	12
8.3	7.8
8.5	8.4
9.8	10.6
9.9	8.6
Mean 8.4	9.3

These were taken in sets of ten, with wonderfully close means, notwithstanding the apparently great probable error.

Another series, with a stronger current, gave as the ratio of the results 1.364.

The results of the investigation are as follows:—

1. By breaking the primary circuit of an induction-coil in a magnetic field, the length of spark produced by the secondary

coil is more than doubled in length. An application of this fact to the induction-coil is suggested.

2. The results by the different methods used all show an increase of electromotive force when the circuit is broken in a magnetic field, and that the effect cannot be purely a mechanical phenomenon as M. Becquerel affirms.

3. By breaking the circuit between mercury and copper in the magnetic field, a remarkable change of polarity was observed with the electrometer.

4. An explanation is offered of the fact noticed by Sir William Thomson, that a greater electromotive force per unit of length is needed to produce a spark at a short distance than at a long one.

The subject of this paper was suggested to me by Professor Trowbridge; and throughout all my work he has kindly given me his advice and help.

Physical Laboratory, Harvard College,
Cambridge, U. S.

LV. *On a Case of Work produced by the Electric Current.*

By R. COLLEY, of Moscow.*

THE notice which I have the honour to place before the readers of the 'Philosophical Magazine' is a succinct *résumé* of two articles which I have recently published in the (Russian) 'Journal of the Physical and Chemical Societies of St. Petersburg' (vol. vii. December 1875, and vol. viii. April 1876)†. For more ample details I refer the reader to the articles themselves, where he will find all the developments and numerical calculations, which I have omitted in the present notice.

The total quantity of energy which becomes free in a determined galvanic element (for instance a Daniell element) by the solution of a gramme of zinc is a constant quantity, independent of the time spent in dissolving the zinc. It is the mechanical equivalent of the chemical reactions that take place in the pile. A portion of this energy always manifests itself under the form of heat in the circuit; another may appear as mechanical work produced by the current. A direct consequence of the principle of the conservation of energy is, that this second portion can only exist and increase at the expense of the first, their sum being constant.

* Translated from the MS. (in French) communicated by the Author.

† An abridged German translation of the first of these articles will be found in Poggendorff's *Annalen*, vol. clvii. p. 370; that of the second will shortly appear in the same journal.

Relying on this principle, and on the laws of Faraday and Joule, we can prove theoretically that any work produced by the current is always accompanied by the appearance in the circuit of a new electromotive force opposed to that of the pile. This result has been confirmed by experiment in all cases hitherto studied: in electromagnetic motors induction-currents weaken the primary current; chemical work, the separation of the elements of a chemical compound, is always accompanied by the manifestation of the electromotive force called "polarization," &c. If, on the contrary, the experiment can be arranged so as to employ external work to reinforce the primary current, the new electromotive force will evidently be of the same sign as that of the pile. This is what would happen if, for example, by the exertion of our arms we were to impress on the electromagnetic motor a motion in the opposite direction to that which it would have taken under the influence of the current.

The aim of the work I have undertaken was, to apply these principles to a case hitherto not investigated, and to confirm by experiment the existence of this force, which cannot at present be classed in any category of known electromotive forces, but must be regarded as a new one.

Let us imagine the following experiment. The current of a Daniell element passes up through a vertical tube filled with any electrolyte (a solution of nitrate of silver, for instance), entering and issuing from the liquid by electrodes of metallic silver, and moreover traverses a galvanometer. A quantity of silver equivalent to the zinc dissolved in the pile will dissolve at the lower and be deposited on the upper electrode. The transport of this metal upward in opposition to its weight constitutes work produced by the current. Nevertheless this work is always accompanied by work of opposite sign, due to the transport in the opposite direction of the group of atoms which in the electrolyte were combined with the metal. This work is manifested by a diminution of concentration of the salt in the immediate vicinity of the negative electrode, and a corresponding augmentation near the positive electrode. Hit-
torf's experiments* on the transport of the products of electrolysis by the current supply the necessary empiric data for calculating the numerical value of this work. It is found that for most salts (among others, for nitrate of silver) this work is less than that of the transport of the metal. There are, however, two iodides, those of zinc and cadmium, in which

* Poggendorff's *Annalen*, vols. lxxxix., xcvi., ciii., cvi. See also Wiedemann, *Galvanismus*, 2nd ed., vol. i. p. 555.

the work of transport of the iodine is greater than that of the transport of the metal.

It is important to observe that, our reasoning being based on the possibility of applying to these phenomena the principle of the conservation of energy, *i. e.* the theorem of *vires vivæ*, the value of the work accomplished is determined solely by the initial and final states of the system of bodies under consideration, and is consequently independent of any possible hypothesis on the nature of electrolysis. As the liquid beyond the immediate vicinity of the electrodes undergoes no alteration, the total work is equal to the algebraic sum of the weights of material transported from one electrode to the other, multiplied by the height of the liquid column. As the phenomenon takes place in the interior of a liquid, the application of the Archimedean principle necessitates the introduction of a correction.

We have seen that all work produced by the current, or supplied by external forces and serving to reinforce the current, is accompanied by the appearance of a new electromotive force in the circuit. In the first case this electromotive force, which I will designate by e , will have the opposite sign, in the second the same sign as that of the pile, E . From this it follows that in a column of nitrate of silver an ascending current will be feebler than a descending one; for, in a circuit of the same resistance, we shall have in the first case the electromotive force $E - e$, and, in the second, $E + e$. The inverse phenomenon will take place with iodide of cadmium. The experiments described below were undertaken for the purpose of verifying this result*.

The details of the numerical calculations will be found in the articles mentioned above. The following are the final values of the electromotive force for a column of 1 mètre:—

Nitrate of silver $e = 0\cdot000005195$ Daniell.

Iodide of cadmium, $-e = -0\cdot000015670$ „

* The experiments were commenced in 1872 (with sulphate of copper). They did not at first promise any success; for this reason I did not publish any thing about them, though I communicated the results to my scientific friends in Moscow. At that period I was obliged, for personal reasons, to suspend for nearly two years all experimental work. In 1873 appeared Prof. Maxwell's book, 'A Treatise on Electricity and Magnetism.' I saw with pleasure that the eminent English physicist had conceived the same idea (I willingly acknowledge that to him belongs the merit of having been the first to enunciate it through the press); to me the coincidence was an additional assurance of the correctness of my theoretic previsions. As far as one can judge from the ten lines devoted by Mr. Maxwell to the subject (vol. i. p. 317), he has calculated the electromotive force e by taking into consideration only the transport of the metal. He does not indicate any experimental means of verification.

This force is independent of the intensity of the current passing through the liquid, and proportional to the height of the column.

The principal difficulty of the experiments was the presence of inconstant and casual currents, due to the inequality of the electrodes, their polarization, &c. The intensity of these currents far exceeded that of the current given by the electromotive force e , if special precautions were not taken to weaken them. I found them much enfeebled by removing the air and gases dissolved in the liquid.

After some trials, I fixed upon the following order of apparatus:—The liquid column was a glass tube filled with the solution of the electrolyte, closed at one extremity, and communicating at the other with an air-pump, through a caoutchouc tube, which could be closed by a pinchcock close to the glass tube. The electrodes were sealed in two lateral tubules, near the extremities. The tube turned on a horizontal axis passing through its centre of gravity, so that either of its ends could be directed upwards. Thus, without any change in the rest of the apparatus, the current could be made to ascend or descend in the liquid column. One of the tubes which I employed, and which I will call No. 1, had a length of 1.6 mètre. Filled with a solution of nitrate of silver, it offered a resistance of 774 Siemens's units. Tube No. 2 had a length of 3.6 mètre, and a resistance nearly double that of No. 1.

The galvanometer was a reflecting one and very sensitive. The wire of the induction-coil was very fine and made a great number of turns; its galvanic resistance amounted to 4077 Siemens's units. Either of the two tubes together with the galvanometer forming a closed circuit, and the sensitiveness of the latter instrument having been determined by preliminary experiments, the deviation due to the electromotive force e acting in a circuit of which the resistance r was known could be calculated beforehand. As I always observed

the difference $\frac{E+e}{r} - \frac{E-e}{r} = 2\frac{e}{r}$ of the descending and the

ascending current, the difference of the deviations produced by these two currents, which we will name δ , was the double of the preceding; and as by "deviation" I mean everywhere the difference of the elongations on both sides of the position of the equilibrium, elongations obtained on changing by a commutator the direction of the current in the galvanometer-coil alone, it is evident that δ was four times a simple deviation due to the current $\frac{e}{r}$. The value of δ is expressed in

arbitrary units, the divisions of the scale, which were observed by reflection in the mirror of the instrument.

Calculation, therefore, gives:—

Nitrate of silver.

Tube No. 1, height 1·6 mètre ...	$\delta_1 = 8\cdot32$
„ „ 2, „ 3·6 „ ...	$\delta_2 = 16\cdot22$

Iodide of cadmium.

Tube No. 2,	$\delta_2 = 43\cdot10$ [$\frac{1}{2}\delta_2 = 21\cdot55$.]
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On passing an external current through my tubes, I soon perceived that the polarization which it produced in the electrodes was so intense and so variable that it was impossible to obtain any result by this method. I therefore resolved to employ instead the current given by the tube itself in virtue of the inequality of the electrodes, their polarization, &c. By precautions, some of which have been described above, this current could be rendered constant and of the same order of magnitude as the current due to the electromotive force e .

In the series of experiments given in the following Tables (pp. 474, 475), I observed alternately the intensity of the proper current of the tube when ascending and when descending through the liquid. Tables I., II., and III. refer to nitrate of silver. Columns 1 and 2 of these Tables contain the elongations of the galvanometer on the current in the latter being changed in direction by a commutator; column 3, their difference, which measures the intensity of the current. A designates the ascending, D the descending current.

Table IV. refers to iodide of cadmium. Considering the intensity of the proper current of the tube in this case, it was inconvenient to change its direction. I therefore observed the excursions only on one side of the position of equilibrium (which was at division 54 for the series of experiments to which this Table belongs). The tube being in a determined position, and the galvanometer having attained a constant deviation, I turned the tube without touching the commutator. I then observed, sometimes a strengthening, sometimes a weakening of the current. It is evident that this method has only half the delicacy of the preceding, and that the difference of the deviations due to the ascending and the descending current is $\frac{1}{2}\delta'_2$. At the foot of each Table are recorded the mean differences D—A or A—D according to the case.

Nitrate of Silver.

TABLE I.

Tube No. 1.

1. D.	27·6	—	73·8	46·2
2. A.	30·2	—	72·1	41·9
3. D.	28·1	—	74·2	46·1
4. A.	31·7	—	73·0	41·3
5. D.	27·6	—	73·4	45·8
6. A.	29·7	—	72·3	42·6
7. D.	28·8	—	74·8	46·0

Difference D — A = 4·1

TABLE II.

Tube No. 1.

1. A.	36·3	—	76·9	40·6
2. D.	32·3	—	78·5	46·2
3. A.	35·6	—	77·1	41·5
4. D.	32·2	—	78·9	46·7
5. A.	33·9	—	76·8	42·9
6. D.	30·9	—	76·8	45·9

Difference D — A = 4·5.

TABLE III.

Tube No. 2.

1. A.	66·9	—	27·5	39·4
2. D.	75·0	—	23·9	51·1
3. A.	66·4	—	26·3	40·1
4. D.	74·8	—	25·3	49·5
5. A.	69·4	—	29·2	40·2
6. D.	78·5	—	28·7	49·8
7. A.	73·7	—	30·8	42·9
8. D.	78·8	—	28·0	50·8
9. A.	74·7	—	30·8	43·9
10. D.	79·7	—	31·4	48·3
11. A.	77·7	—	33·4	44·3
12. D.	83·3	—	33·6	49·7
13. A.	80·0	—	36·7	43·3
14. D.	85·1	—	35·8	49·3
15. A.	80·2	—	37·7	42·5
16. D.	85·0	—	37·3	47·7
17. A.	81·8	—	39·3	42·5
18. D.	87·1	—	39·4	47·7
19. A.	83·1	—	40·3	42·8
20. D.	87·5	—	40·4	47·1

Difference D — A = 6·9.

Iodide of Cadmium.

TABLE IV.

Tube No. 2.

			A - D.
1. A.	192.4	D. 185.8	6.6
2. D.	182.0	A. 189.3	7.3
3. A.	188.4	D. 181.5	6.9
4. D.	183.0	A. 190.3	7.3
5. A.	190.5	D. 184.8	5.7
6. D.	183.3	A. 190.5	7.2

Difference A - D = 6.8.

The previsions given by the theory are, then, confirmed by experiment, inasmuch as a difference is shown between the intensities of the ascending and the descending current in a column of an electrolyte, and the sign of the difference accords with that indicated by the theory. In nitrate of silver the descending current is stronger than the ascending; the reverse is the case with iodide of cadmium.

But the numbers given by experiment are twice or three times less than those foreseen by the theory; it is impossible to attribute so enormous a difference to errors of observation. The most probable explanation of this apparent anomaly is to be sought in a peculiarity of feeble currents noted by Helmholtz*. Feeble currents, which polarize the electrodes without producing definite decomposition of the electrolyte (their electromotive force being less than the electromotive force of polarization which they would call forth in passing through the liquid), or the currents which arise from the depolarization of the electrodes, present according to M. Helmholtz the following peculiarity:—Ohm's law seems to be inapplicable to them: a very great metallic resistance introduced into the circuit weakens their intensity but very little, which amounts to the same as if the liquid or the electrodes were the seat of an enormous supplementary resistance. Experiment proves that this fictitious resistance is variable and augments when the intensity of the current diminishes. These phenomena present a striking analogy with the currents given by electrical machines. M. Rossetti's experiments on the Holtz machine† have shown that what may be called the internal resistance of that machine in like manner varies with the velocity of rotation, consequently with the intensity of the current. My tubes presented this peculiarity in a very high

* Poggendorff's *Annalen*, vol. cl. p. 483.

† D'Almeida's *Journal de Physique*, 1875; Pogg. *Annalen*, vol. cliv. p. 507.

degree; it is therefore not surprising that the deviations of the galvanometer, calculated on the basis of the real value of the resistance of the circuit alone, should be less, when, besides this real resistance, one must take into account a very considerable fictitious resistance.

This circumstance not having attracted my attention at the commencement of the experiments, I cannot present the data necessary for calculating the value of this fictitious resistance in the experiments with nitrate of silver. For those with iodide of cadmium (Table IV.) I determined this resistance by measuring the intensity of the current, first, when the tube and the galvanometer alone formed the circuit, and again after the introduction of a coil of 7976 Siemens's units. I found that the total resistance of the circuit was 18,240 units, while the real resistance was only about 5600. The galvanometer-deviation A—D, calculated with this new datum, was found to be 6.6, in complete accordance with the experiments.

The electromotive force e , tending to produce a current directed invariably downward in a column of nitrate of silver, and upward in iodide of cadmium, is independent of the intensity and direction of the current of the pile. I endeavoured to pass through my tubes filled with nitrate of silver equal currents alternately in opposite directions, supplied by a magneto-electric machine. These currents, without acting themselves on the galvanometer, were to call forth the electromotive force e , which was to deflect the needles.

Contrary to expectation, the experiment gave a negative result; the phenomena were the same as if the alternate currents had not traversed the liquid at all. The reader is referred for fuller details to the articles above mentioned. There is reason to think that the quantity of electricity (necessarily *finite*) conveyed by any induced current is insufficient to charge the electrodes to a difference of potential above the maximum electromotive force of polarization, as, according to Mr. Varley* and Helmholtz, the electrodes may be regarded as the armatures of a condenser of enormous capacity. Now, under these circumstances, although the true electromotive force of the induced current may at every instant be superior to the maximum of polarization, the current cannot pass through the liquid. This complex and little-studied subject requires fresh researches for its complete elucidation.

In the phenomena known under the name of "electrical

* Philosophical Transactions, vol. clxi. p. 129 (1871).

endosmose" * the current may also produce work, when the pressures are different on the two sides of the diaphragm. It is easy to prove that the electromotive force e which corresponds to this case is that of the "diaphragm-currents" discovered in 1859 by Quincke †.

Moscow, April 1876.

LVI. *Application of the Mechanical Theory of Heat to the Study of Volatile Liquids: Simple Relations between the Latent Heats, Atomic Weights, and Tensions of Vapours.*
By RAOUL PICTET ‡.

[Plate IV.]

IN almost all recent works on the mechanical theory of heat the authors devote the first chapters to the general equations of the thermic problems—that is to say, to the principle of equivalence, and to the second principle, that of the equality of restoration in reversible cycles; they then apply directly the general equations found to the changes of state of bodies, and to the phenomena of calorimetry connected with them. Sady Carnot, Clapeyron, and afterwards Clausius, Dupré, Zeuner, Hirn, &c., have apparently followed analogous courses and been conducted to very similar results. Nevertheless the physical solution of the problems analytically investigated is always obscure; it cannot get rid of the confused train of equations which constantly veil a positive comprehension of the results of the analysis.

It is, in great measure, in consequence of an abuse of formulæ which are not translated into ordinary terms that treatises on physics give no information upon the relations which connect the various properties of volatile liquids. The aim we propose to ourselves is, to give some laws representing with sufficient exactness the notions at present held on this question.

First let us settle the principal factors of the physical problem.

We have a kilogramme of any liquid whatever, at the temperature t° . At this temperature it is caused to pass from the liquid into the gaseous state; in doing this it absorbs a quantity of heat represented by λ calories—the latent heat of

* These phenomena, discovered by Prof. Fr. Reuss, of Moscow, were investigated chiefly by Wiedemann, Pogg. *Ann.* vol. lxxxvii.; see also *Galvanismus*, vol. i. p. 576.

† Pogg. *Ann.* vols. cvii. & cxiii. Wiedemann, *Galvanismus*, vol. i. p. 946.

‡ Translated from the *Bibliothèque Universelle, Archives des Sciences Physiques et Naturelles*, Jan. 15, 1876, vol. lv. pp. 66–83.

volatilization; and finally the volume v of the liquid becomes volume V of the vapour. During the passage from the liquid to the gaseous state the temperature t° has been kept constant, and the tension of the vapour, P , has been likewise constant.

Now, if all known liquids be taken and made to undergo the same transformation at the same temperature t° , we find for each different numeric values of λ , P , and V . For each liquid the maximum tension P , the volume V , and the latent heat λ vary; and the deviations may be very considerable. We may therefore inquire whether these three quantities are independent variables, to be determined *empirically* for each liquid, or if any relation permits them to be expressed in terms of one another.

M. Régnault, in his large and splendid work on the maximum tensions of volatile liquids and their latent heats of volatilization, has considered the question only from an experimental point of view, and has given interpolation-formulæ for the tensions and latent heats, without seeking to establish any theoretic ratio between these two factors. The interpolation-formulæ are often most complex, and do not enable us to distinguish any thing general when the different liquids are compared. His copious statement, however, of the numbers obtained by accurate experiments will serve as a control in the following theoretical discussion.

In order to put the problem in a complete, definite, and readily comprehensible form, let us suppose we have a reservoir, A (Pl. IV. fig. 1), containing any volatile liquid. A pump, B, draws off the vapour, which forms in the reservoir A at a constant temperature t° and tension P , and forces it into the receiver C at a temperature t' and under a constant pressure P' . The volatile liquid will pass from the liquid into the gaseous state in the reservoir A, afterwards from the gaseous into the liquid state in the reservoir C. To maintain the operation and complete the cycle, the liquid is to be brought back to its initial temperature; and the conduit tube D permits the liquid accumulated in C to return into the first reservoir, A. We suppose in the above operation $t' > t^\circ$, and consequently $P' > P$.

Thus, for example, we take 1 kilogramme of ether at $t^\circ = 0^\circ \text{C.}$; its vapour is drawn off under the tension P_0 , and forced into C at a temperature $t' = 20^\circ$ and under a pressure P'_{20} ; and then 1 kilogramme of liquid ether is reduced from the temperature 20° to 0° in passing through the tube D. The cycle is complete, since the ether has returned to 0° under the initial pressure P_0 , which was the point from which it started.

We can thus calculate, by means of the numbers and Tables furnished by M. Régnault, two essential elements of the problem :—(1) the heat absorbed in A by the volatilization of the liquid, minus the heat restored by the return of the liquid from the temperature t' to t° ; (2) the work expended, by the action of the pump B, to obtain the compression of the vapour from the pressure P_0 to P'_{20} .

Each of these two equations is completely independent of the other; and one of them gives a quantity of calories, the other a quantity of kilogrammètres.

The problem has not hitherto been stated in this form; and yet it is the natural method for arriving at the discovery of the connexion that may exist between the quantities which have been supposed to be entirely independent and without any intimate relation between them.

For the calculations we will adopt the following symbols :— t° , temperature of the refrigerant A; t' , temperature of the condenser C; $\frac{1}{274}$, the coefficient of dilatation of the gases; P, the maximum tension of the vapours at t° ; P' , the maximum tension of the vapours at t' ; T, the work done by the pump B; c , the specific heat of the liquid; δ , density of the vapour at 0° , referred to that of the air; 1.293 kilog., weight of a cubic mètre of air; 10333 kilog. = atmospheric pressure on a square mètre; λ , the latent heat of the liquid at t° .

We will suppose that the laws of Mariotte and Gay-Lussac apply rigorously, which is not strictly correct for all vapours. We make the calculation for 1 kilog. of liquid.

1st. The work T.

The work is produced by the compression of the vapour at t° when it is made to pass from the pressure P to the pressure P' . By calculation we find for T :—

$$T \text{ kilogrammètres} = \frac{10333 (274 + t^\circ) l \left(\frac{P'}{P} \right)}{1.293 \delta \times 274} \dots (I.)$$

The expression $l \left(\frac{P'}{P} \right)$ represents the Napierian logarithm of the quotient of the pressures; it is introduced into this formula by integrating the work between the limits P and P' .

2nd. The heat absorbed in the refrigerant A.

For 1 kilogramme of liquid, we have

$$\lambda - c (t' - t) = \text{heat absorbed in A, in calories.} \dots (II.)$$

Note that these two equations have not one letter in common; they represent two totally different orders of facts.

Indeed, let us suppose we have two volatile liquids possessing the same density, the same tensions P and P' , the same specific heat c , but one of them having twice the cohesion of the other. The internal work of disgregation of the one will be double that of the other; and consequently the latent heat λ will be twice as much as λ' , since latent heat is nothing else but that which is absorbed in order to overcome the cohesion of the liquid molecules.

In this example the quantity T would be the same for both liquids; but the second formula would give for $\lambda - c(t' - t)$ and $\lambda' - c(t' - t)$ values which might vary from the single to the double.

Let us suppose, in the second place, that the two liquids have λ and λ' equal, c equal, but $\delta' = 2\delta$. The quantity, in kilogrammètres, expressed by T and T' will be

$$T = \frac{10333 (274 + t) l \left(\frac{P'}{P} \right)}{1.293 \delta \times 274},$$

$$T' = \frac{10333 (274 + t) l \left(\frac{P'}{P} \right)}{1.293 \times 2\delta \times 274},$$

replacing δ' by 2δ ; thus $T = 2T'$, while the heat withdrawn from the refrigerant A will be constant in the two cases.

It may likewise be that all the quantities λ , c , P , P' , δ , &c. vary from one liquid to the other, in such manner that the equations have nothing general as a theoretic result, and that they can only be applied for each liquid in particular by taking the numerical coefficients from experimenters worthy of confidence.

Therefore, forming no hypothesis on the constitution of liquids, but accepting the *arbitrary* and *empiricism* in this matter, our two equations remain *essentially independent*.

It is here that an *hypothesis* finds a place; and we emphasize the word, because it is purely and simply an hypothesis: *we suppose that the cohesion of liquids is constant for all*; we assume that the mobility of the molecules which characterizes the liquid state corresponds to an equal molecular or atomic attraction—that λ and λ' , referred to two molecules, are equal and constant for all liquids without exception. This first hypothesis furnishes a relation between equations I. and II.

We will now make a second hypothesis, viz. that Carnot's cycle applies to volatile liquids and to their changes of volume when they are volatilized, and that it establishes the ratio between the work expended and the heat absorbed.

Of these two hypotheses, therefore, the first is based on a deduction resting upon the similar physical state of liquids, the second on the principle of equality of return.

If the second hypothesis were not verified, we might combine two reversible cycles, Carnot's and that supplied by a volatile liquid; the difference of the two would be represented by the transformation of a certain quantity of surrounding heat into mechanical work. In fact, Carnot's cycle proves that, given a quantity of heat Q disposable at temperatures t' and t , we can always transform into work a quantity

$$Q = \frac{t' - t}{274 + t'} \text{ calories.}$$

In the cycle represented in our figure 1, we dispose in the condenser of a quantity of heat of which the minimum is

$$Q = \lambda \text{ at a temperature } t';$$

we can therefore always derive from it a motive force given by the expression

$$T = \lambda \frac{t' - t}{(274 + t')} \times 433.5 \text{ mechanical heat-equivalents.}$$

If this quantity be greater than T derived from equation I., we shall evidently have disposable work which will be expressed by the combination of the two cycles, or $T' - T$, and which will be derived from surrounding sources of heat.

Introducing our two hypotheses into equations I. and II., we have the following relations:—

Our equation II. may be represented by

$$\lambda - c(t' - t) = Q, \text{ heat absorbed in A.}$$

If we calculate by means of Carnot's cycle the work which must be expended in order to derive a quantity of heat Q at the temperature t and make it pass into t' , it is given by the expression

$$Q' - \frac{Q(t' - t)}{274 + t'} = \lambda - c(t' - t) = Q.$$

Reducing this equation, and denoting by E the mechanical equivalent of the heat, we get

$$\frac{[\lambda - c(t' - t)]E(t' - t)}{274 + t} = \text{kilogrammètres required.}$$

We now equate this work obtained by Carnot's cycle with that which we found for the cycle of a volatile liquid, and

thence derive the following principal relation,

$$\frac{10333(274+t)l\left(\frac{P'}{P}\right)}{1.293\delta \times 274} = \frac{[\lambda - c(t'-t)]E(t'-t)}{274+t}. \quad (\text{III.})$$

Deriving from this equation the value of $\lambda - c(t'-t)$, we get, as the general formula corresponding to our hypothesis,

$$\lambda - c(t'-t) = \frac{10333(274+t)^2 l \left(\frac{P'}{P}\right)}{1.293\delta E \times 274(t'-t)}. \quad (\text{IV.})$$

If our hypothesis is correct, if it answers to the physical conditions of the problem, it will prove true for all liquids and at all temperatures. We must take any liquid whatever, select arbitrarily two temperatures t and t' , seek in M. Régnault's Tables the pressures P and P' corresponding to the temperatures t and t' , and ascertain if equation IV. is constantly accurate. If the results do not confirm our previsions, it is because the hypothesis is ill-founded and Carnot's cycle does not apply to volatile liquids.

We will take water for example, which was so carefully studied by M. Régnault, and verify the equation for the temperatures $t=100^\circ$ and $t'=110^\circ$. The constants are $P=760$, $P'=1075.37$, $\delta=0.625$, $c=1$, $E=433.5$, $\frac{P'}{P}=1.41496$.

$$\lambda - c(t'-t) = \frac{10333(374)^2 l (1.41496)}{1.293 \times 0.625 \times 433.5 \times 10 \times 274}.$$

Here is the calculation *in extenso* as an example of a particular case:—

$$\text{Log } 1.41496 = 0.1507400.$$

Log 0.1507400 = 1.1782285	Log 1.293 = 0.1115985
„ $\frac{1}{m}$ = 0.3622157	„ 0.625 = 1.7958800
„ 10333 = 4.0142264	„ 433.5 = 2.6369891
„ 374 = 2.5728716	„ 274 = 2.4377506
„ id. = 2.5728716	„ 10 = 1.0000000
	5.9822182
Log numerator... 8.7004138	
Log denominator 5.9822182	
2.7181956 log 522.63.	

And we have $\lambda - 10 = 522.63$, whence $\lambda = 532.63$.

Looking into the Tables of M. Régnault, we find that, for the temperature of 100° , λ is equal to 536 calories. The dif

ference is only 3 calories, or $\frac{1}{200}$ of the real value. This result, then, confirms and justifies our hypothesis.

Equation IV. differs essentially from those which have been proposed by MM. Clapeyron, Clausius, Zeuner, &c. by the term $c(t'-t)$, which has been totally neglected in the analysis of the problems belonging to the investigation of latent heats.

Before exhibiting the general Table of the results to which this method of calculation leads, let us simplify equation IV. by investigating the latent heat of liquids at the boiling-point. For the temperature of ebullition the pressure P becomes the same for all liquids; and if we make t' converge towards t , P' will come indefinitely near to P; therefore $t'-t$ converges

towards 0, and the fraction $\frac{l\left(\frac{P'}{P}\right)}{t'-t}$ becomes the derivate of the pressure in relation to the temperatures. Equation IV. can then be written

$$\lambda = \frac{10333(274+t)^2}{1.2938E \times 274} \times \text{derivate of } \frac{l\left(\frac{P'}{P}\right)}{t'-t}.$$

To obtain this derivate, let us differentiate the equations of the pressures expressed as functions of the temperatures in immediate proximity to the boiling-point. In fig. 2 we have represented the whole of the curves given by M. Régnault. The abscissæ are reckoned from one 5 degrees to another above and below the boiling-point; and the ordinates are the corresponding pressures. This curve shows clearly the result of the differentiation—namely, that we arrive at a derivate which is sensibly a *constant* multiplied by a simple function of the absolute temperature for all liquids. The curve widens slightly above and below the pressure of 760 millims.; but in the immediate vicinity of that pressure the same curve represents very accurately all liquids.

In the following Table we put the names of the volatile liquids in the first column, in the second the temperatures of the boiling-point, in the third the calculated latent heats, and in the fourth the latent heats observed by M. Régnault and given in his work. For very volatile liquids the derivate increases very sensibly, as may be seen. We give here only a few liquids from the general Table.

Liquid.	Boiling-point.	λ calculated.	λ observed.	Derivate.
Water.....	100°	533.9	536	0.0346
Alcohol	78.21	210	214.05	0.0325
Sulphide of carbon	46	84	83.54	0.03189
Sulphuric ether.....	35	90.12	89.76	0.03563
Chloroform	60	60.73	61	0.03337
Benzine	80	92.9	92.26	0.03000
Chlorhydric ether.....	16	92.8	92.1	0.03779
Essential oil of turpentine ...	155	65.6	68	0.0261
Mercury.....	350	71.6	77	0.02049
Sulphurous acid	- 10	94.2	94.5	0.04374

It is easy to see the perfect coincidence which exists between the results of our hypothesis and those furnished by experiment. The deviations are very slight, and are readily explained by the anomalies of Mariotte's law and the errors of observation, of which no account was taken in our equations.

The column of derivatives shows that the value of $\frac{l\left(\frac{P'}{P}\right)}{t'-t}$ for $P=760$ and $t'=t$ increases with the volatility of the liquids; we can represent with sufficient exactness the values of this derivate by

$$D \frac{l\left(\frac{P'}{P}\right)}{t'-t} = K \frac{274}{274+t} \quad \dots \quad (V.)$$

Applying this formula (V.) to the liquids mentioned in the preceding Table, we have:—

Liquid.	Boiling-point.	Derivate.	Value of K.
Water	100°	0.0346	0.045
Alcohol	78.21	0.0325	0.042
Sulphide of carbon	46	0.03189	0.039
Sulphuric ether.....	35	0.03563	0.040
Chloroform	60	0.03337	0.040
Benzine	80	0.03000	0.040
Chlorhydric ether.....	16	0.03779	0.040
Essential oil of turpentine ...	155	0.0261	0.040
Mercury	350	0.02049	0.044
Sulphurous acid	- 10	0.04374	0.041

Thus the factor $\frac{274+t}{274}$ multiplying the derivate of the pressures with respect to the temperatures gives a constant quan-

tity 0.041, or very nearly. For liquids so different as mercury and sulphurous acid, the difference is only 0.003, or a deviation of less than $\frac{1}{13}$.

Having verified our hypothesis concerning Carnot's cycle and that of volatile liquids, we return to our hypothesis relative to the cohesion of liquids. We suppose that *atomic cohesion is constant* for all substances affecting the liquid state. Let α denote the weight of a liquid atom, and F the cohesion which connects it with those surrounding it; let P be the pressure at the temperature t° ; and let us choose for all liquids a constant pressure $P = 760$ millims., which corresponds to the boiling-point.

Since the external pressure, exerted by the vapour on the liquid, is constant, *the heat-effort and its work of disgregation is exactly represented by the internal latent heat at that temperature*. Now, if F is the force acting according to any law through a space equal for all liquids, the work of disgregation to separate into the gaseous state an atom of liquid will be $Fk = \text{a constant}$.

The number of atoms contained in a weight 1 is $\frac{1}{\alpha}$; but α must be taken at its value for the temperature t —that is to say, the density of the atom variable according to the temperature of the boiling-point. Integrating the elementary work for the weight of 1 kilogramme, the sum will represent the internal latent heat of the liquid at the temperature t° , and we have the fundamental equation

$$\lambda = \frac{K(274+t)}{\alpha \times 274}, \quad \dots \dots \dots \text{(VI.)}$$

whence we deduce

$$\frac{274\alpha\lambda}{274+t} = K; \quad \dots \dots \dots \text{(VII.)}$$

and making the constant factor 274 to vanish for all liquids, we arrive at

$$\frac{\alpha\lambda}{274+t} = K, \quad \dots \dots \dots \text{(VIII.)}$$

an equation connecting the atomic weight with the temperature and the internal latent heat.

Before making the numerical verification, we will remark that the external latent heat is sensibly proportional to α ; so that the equation (VIII.) will refer with sufficient accuracy either to the total latent heat, or to the internal latent heat alone. In the following Table we give the products of the atomic weight by the two latent heats.

Liquid.	Total latent heat.	Boiling-point.	Atomic weight.	External latent heat.	Total heat by atomic weight.	Internal by atomic weight.
Water	536 (cal.)	100°	9	40 (cal.)	12·50	11·80
Chloroform	61	60	59	5·31	10·80	10·00
Sulphide of carbon	83·54	46	38	8·18	10·00	9·70
Sulphuric ether	89·76	35	37	8·15	10·7	10·05
Essential oil of turpentine	68·8	155	68	5·14	11·8	10·90
Mercury	77·5	350	100	6·06	11·9	10·2
Acetone	130	55	29	11·044	11·45	10·48
Benzine	92·26	80	39	8·838	10·17	9·98
Wood-spirit	264	67	16	20·48	12·35	11·30
Chlorhydric ether	92·1	10	32	8·58	10·37	9·65
Alcohol	210	78·2	23	14·9	12·5	11·5
Sulphurous acid	94·56	-10	32	8·65	11·2	10·4

This Table shows, by the numbers contained in the last two columns, that all the liquids have the same cohesion, and that our hypothesis is true. These comparisons will perhaps make it possible for us to calculate as a fraction of a millimetre the real diameter of the atoms. Indeed cohesion plus the pressure exerted upon the liquid are the only forces which resist the disgregating action of heat; and by varying the temperature we get two numerical equations which would be perfectly definite, if we knew the law of the attractions as functions of the distances of the attracting particles, and the distribution of the external pressure upon each atom. I reckon upon resuming this question in an early publication.

The result of this numerical verification permits us to establish another relation between the latent heats and the atomic weights.

Since cohesion is a constant quantity for all liquids, what do the differences observed by M. Régnault in the internal latent heats at various temperatures represent? They are only the expression of the work expended to separate the constituent atoms of the liquid from the temperature t to t' . If we admit that the temperatures are proportional to the amplitudes of the flight of the atoms from each other, calling F the attraction

of two atoms for a distance corresponding to 1° , $\frac{F}{x^m}$ = the attraction for a temperature x , and the integral of the work necessary to raise the temperature from t to t' will be $F \int_t^{t'} \frac{dx}{x^m}$. This elementary work must be repeated for each atom of the liquid; and referring the work to the kilogramme we shall have $\frac{1}{\alpha} \int_t^{t'} \frac{dx}{x^m}$ = work expended to raise the temperature

of the liquid from t to t' . Dividing this quantity by the mechanical equivalent 433·5, we shall obtain the differences of the latent heats, and hence equation IX.,

$$\frac{F}{433\cdot5\alpha} \int_t^{t'} \frac{dx}{x^m} = \text{difference of the internal latent heats.} \quad (\text{IX.})$$

But we have demonstrated that cohesion is the same for all liquids; therefore the factor $\frac{F}{433\cdot5} \int_t^{t'} \frac{dx}{x^m} = K$ for all liquids, and equation IX. is simplified into,

$$\text{Difference of the internal heats} = K\alpha. \quad (\text{X.})$$

From this equation we conclude that, for two temperatures t and t' taken arbitrarily, *the difference of the internal latent heats, multiplied by the atomic weight, is a constant number for all liquids.*

The following is a statement of some numerical verifications with the differences of temperature 100° and 110° , 100° and 150° , 50° and 150° :—

Liquid.	Internal latent heats.			Atomic weight.	Product.
	At 100° .	At 110° .	Difference.		
	calories.	calories.	calories.		
Water	496·29	488·36	7·93	9	71·37
Alcohol	183·60	177·21	6·39	11	70·29
Chloroform	51·03	49·86	1·17	59	69·03
Chloride of carbon ...	39·64	38·53	1·11	65	72·15
Sulphide of carbon ...	67·33	65·34	1·99	38	75·62
	At 100° .	At 150° .			
Water	496·29	456·70	39·59	9	356·31
Alcohol	183·60	154·09	29·51	11	324·61
Chloroform	51·03	45·12	5·91	59	348·69
Chloride of carbon ...	39·64	33·76	5·88	65	382·20
Sulphide of carbon ...	67·33	56·61	10·72	38	407·36
	At 50° .	At 150° .			
Water	536·12	456·70	79·42	9	714·70
Alcohol	218·75	154·09	64·66	11	811·06
Chloroform	56·75	45·12	11·63	59	686·17
Chloride of carbon ...	44·64	33·76	10·88	65	707·20
Sulphide of carbon ...	76·07	56·61	19·46	38	739·48

Of all experimental determinations, those of internal latent heats are the most delicate in calorimetry; so that we cannot demand more complete accord between theory and experiment within limits of temperature so wide apart; the agreement is sufficient to justify our law respecting the internal latent heats.

As a complement to the experimental synthesis of our equations, we can apply them to a cycle similar to that of

fig. 1, which has been employed industrially in the production of ice by means of sulphurous acid. The observations were made with extreme accuracy during the space of one month at La Ciotat, near Toulon, by the care of M. Vésigné, Manager of the dockyards of the Messageries Maritimes.

One engine produced 36 kilogrammes of ice per hour, under the following conditions:—

Temperature of the refrigerant -10° ; temperature of the condenser $+35^{\circ}$.

Pressure at $-10^{\circ} = 762.49$ millims.; pressure at $35^{\circ} = 4014.78$ millims.

Specific heat $c = 0.35$; λ at -10° for sulphurous acid $= 102$; and $\delta = 2.112$.

The equation of the quantity of heat absorbed in one minute to produce 36 kilos of ice per hour is given by

$$P \cdot 102 - p \cdot 45 \times 0.35 = 60 \text{ calories,}$$

in which p is the weight of sulphurous acid volatilized. We find $p = 694$ grammes. Substituting this value of p in the equation of the work T for 1 second, we have

$$T = \frac{10333 \cdot p \cdot l \left(\frac{4014.78}{762.49} \right)}{1.293 \times 2.112 \times 60} = 69.44 \text{ kilogrammètres.}$$

We will remark that ice proceeding from the solidification of water at 21° absorbs 100 calories per kilogramme; consequently the engine absorbs 1 calory per second; and by Carnot's cycle we have the following relation,

$$T = \frac{1(t' - t)E}{274 + t} = \frac{45 \times 433.5}{309} = 63.13 \text{ kilogrammètres.}$$

By the cycle of sulphurous acid we find 69.44 k.; the coincidence is almost absolute. Let us add that in practice the ice-engine consumes under these conditions one horse-power of steam, or about 75 kilogrammètres.

From all that precedes we can draw the following conclusions:—

1. *Cohesion is a constant quantity for all liquids.*
2. *The derivate of the Napierian logarithm of the quotient of the tensions by the temperatures is constant for all liquids at the same pressure and the same temperature.*
3. *The latent heats of all liquids, brought to one and the same pressure, multiplied by the atomic weight at the same temperature, give a constant product.*
4. *For all liquids, the difference of the internal latent heats at any two temperatures, multiplied by the atomic weight, is a constant number.*

We see now that the quantities which seemed absolutely independent of each other are united by relations all very simple, rendering unnecessary the employment of long empiric formulæ, which often rest only on more or less questionable observations.

Finally, if we compare the results of this investigation with Dulong and Petit's law on specific heats, we can establish one relation more, and say that *the latent heats of all liquids are multiples of the specific heats.*

In a memoir to be published shortly, we shall more completely develop these questions, of which these few lines are only a succinct epitome.

LVII. Notices respecting New Books.

The Theory of Screws: A study in the Dynamics of a rigid body. By ROBERT STAWELL BALL, LL.D., F.R.S., Andrews' Professor of Astronomy in the University of Dublin, and Astronomer Royal of Ireland. Dublin: Hodges, Foster & Co., Grafton Street. 1876 (8vo, pp. 194).

THIS book contains the substance of various previously published papers, which the author has rewritten and systematically arranged. It is in fact a treatise on the equilibrium and motion of a rigid body, regarded from a point of view quite different from that of other writers.

The theory is worked out with great completeness and in a manner that bears conspicuous testimony to the extent of the author's acquirements and to his power as a mathematician.

For several reasons it is difficult to convey any idea of the contents of the work within the limits of a short notice. Thus, to mention only one—very many technical terms are used, which need definition for the purposes of the discussion in hand; a list of fifty-five of these terms is given in a separate index. We will, however, endeavour to state, as briefly as possible, some of the fundamental points of the theory, and to show by an example or two how they apply to the exposition of the subject. It will be proper to premise that the discussion is conducted under certain restrictions; *e.g.* the body is supposed to be acted on by the same forces whenever it occupies the same position—a restriction which excludes such forces as those arising from a resisting medium, or from friction. Again, it is assumed that the body while under examination continues indefinitely adjacent to its original position, so that all displacements are supposed to be small. Though this restricts the field of inquiry, it still leaves it wide enough to include the theory of equilibrium, of impulsive forces, and of small oscillations.

It is well known that any displacement which a rigid body undergoes may be reduced to a rotation round an axis and a translation

parallel to that axis, and that any system of forces acting on a rigid body can be reduced to a single force and a couple whose plane is at right angles to the direction of the force. These theorems are the starting-point of Theory. Adverting, in the first instance, to the former, or kinematical theorem, it will be observed that the displacement of the body might be effected by supposing it to be rigidly attached to a screw working in a fixed nut, the axis of the screw being properly directed and its pitch properly chosen. For the purpose of the present discussion the term pitch denotes the distance through which the screw advances when turned through the unit angle of circular measure; indeed a screw is defined by our author to be "a straight line in space with which a definite linear magnitude, termed the pitch, is associated." Accordingly the theorem may be stated thus:—any displacement of a rigid body may be represented by a *twist about a screw*. In like manner, the second or mechanical theorem above cited may be stated thus:—Any system of forces acting on a rigid body may be reduced to a *wrench on a screw*,—meaning thereby a force directed along the axis of the screw and a couple acting in a plane at right angles to the screw, whose moment is the product of the force and the pitch. It is hardly necessary to observe that the reductions contemplated in these two theorems are unique.

The composition of twists and wrenches can be effected by the same method; but we shall state it with reference to the former. Let the axes of x and y be two screws (α and β) whose pitches are p and q , and suppose the body to receive simultaneously about them twists whose amplitudes are $\theta \cos l$ and $\theta \sin l$, it can be shown that these are equivalent to a single twist of amplitude θ about a screw whose position is defined by the equations

$$y = x \tan l, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

$$z = (p - q) \sin l \cos l, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

and whose pitch is

$$p \cos^2 l + q \sin^2 l. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

If we eliminate l between (1) and (2) we obtain

$$z(x^2 + y^2) - (p - q)xy = 0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

This is, of course, the equation to the surface which is the locus of the screws, the twists about which are the resultants of two twists, of all relative magnitudes, simultaneously effected round α and β . It will be observed that each generator of the surface has its own particular pitch given by (3), an expression which can be represented by an obvious geometrical construction. The surface is called the *cylindroid*: a method of constructing a model of it is given on p. 193; and a representation of the model when constructed forms a frontispiece to the volume.

The above method gives a rule for the composition in only a particular case. It is completed, however, by observing that if we have any two assigned screws (θ and ϕ) placed anyhow in space, one cylindroid (and only one) can be drawn to contain them, and

which they determine, the pitch corresponding to each generating line (it must be remembered) being also determined. It can be shown that if a body receive twists about three screws on a cylindroid, and if the amplitude of the twist about any one screw is proportional to the sine of the angle between the other two screws, the body will return to its original position. Hence, supposing the cylindroid determined by two screws to be constructed, we have a simple general rule for the composition of two twists about those screws. And the like is true of wrenches.

Let there be two screws whose pitches are p and q inclined to each other at an angle O , and let d denote the shortest distance between them; suppose the body to receive a wrench of intensity P round the latter and to twist round the former through an angle of small amplitude α ; the work can be proved to equal

$$\alpha P \{ (p+q) \cos O - d \sin O \} :$$

the coefficient of αP is called the virtual coefficient of the two screws. It is plain that if the constants are so related that

$$(p+q) \cos O - d \sin O = 0$$

no work will be done by P , and consequently that the two screws p and q have this property, that a body at rest, but free to twist round the former, remains at rest when it receives a wrench round the latter. The form of the above expression shows that if the body is at rest, but free to twist round the latter screw (q), it will remain at rest when it receives a wrench round the former (p). Two screws (p and q) thus related are called reciprocal screws.

Now suppose a screw (η) to be reciprocal to two screws (θ and ϕ), it will be reciprocal to every screw in the cylindroid (θ, ϕ); η will cut the cylindroid in three points, and therefore will meet three screws, to one of which it will be perpendicular; and the other two will have pitches equal and of opposite sign to its own. Let any point P be taken, and from it let lines be drawn at right angles to the screws of a cylindroid severally; any one line meets the surface on two other screws of equal pitches; let a pitch equal and of opposite sign to them be attributed to it; the locus of the lines (which are proved to form a cone of the second order) is the reciprocal cone drawn through that point to the cylindroid.

The points we have hitherto mentioned relate to the foundation of the method, and receive considerable development before it is applied to the discussion of the dynamics of a body enjoying different orders of freedom. How the method is applied may be in some degree understood from the following general statement regarding the contents of the latter part of the volume. In the discussion of each order of freedom "we shall first ascertain" (says the author) "what can be learned as to the kinematics of a rigid body, so far as small displacements are concerned, from merely knowing the order of the freedom which is permitted by the constraints. This will conduct us to a knowledge of the screw complex, which exactly defines the freedom enjoyed by the body. We shall then be enabled

to determine the *reciprocal screw complex*, which involves the theory of equilibrium. The next group of questions will be those which relate to the effect of an impulse upon a quiescent rigid body, free to twist about all the screws of the screw complex. Finally, we shall discuss the small oscillations of a rigid body in the vicinity of a position of stable equilibrium, under the influence of a given system of forces, the movements of the body being limited as before to the screws of the screw complex" (p. 83).

An explanation of the meaning of the technical terms printed in italics is needed for the elucidation of this passage. In the first place suppose a body to be free to receive a twist about each of n screws, and let it receive any twist about each of them, its final position could have been reached by a twist round some screw which is merely one of an infinite number about which it is possible for the body to twist. All these screws together with the original n screws is called a *screw complex* of the n th order; and if a body cannot be twisted about any screw but one belonging to this complex, it is said to have *freedom* of the n th order. It is to be observed that n cannot exceed six; so that a body can only have six different orders of freedom. Now if we suppose a screw to be reciprocal to n of the screws of a screw complex of the n th order, it will be reciprocal to every screw of the complex. And all these screws will form the *reciprocal screw complex*. If the screw complex is of the n th order, the reciprocal screw complex is of the $(6-n)$ th order. Of course, if the freedom of the body is expressed by any assigned screw complex, that body will remain at rest when acted on by a wrench round any screw of the reciprocal screw complex.

Consider, for instance, the case of the equilibrium of a body which has freedom of the second order. In this case the body is free to twist round two screws θ and ϕ , and therefore round any screw of the cylindroid (θ, ϕ) . This cylindroid is therefore the *screw complex* of the *second order*. Through each point of space (as we have seen) a cone of reciprocal screws can be drawn. The author speaks of each of these cones as a reciprocal screw complex of the fourth order, one for each point in space; but, of course, all of them together form the reciprocal screw complex of the fourth order according to the definition above cited. If any one of these cones is considered, each generating line has its own proper pitch, and the pitches have every value from $+\infty$ to $-\infty$; so that one screw of given pitch belonging to a screw complex of the fourth order can be drawn through each point in space. Of course it follows, from what has been stated above, that if a body, being at rest and having freedom of the second order, receive a wrench round any one of these screws, it will remain at rest. Suppose a force P to act on the body along an assigned line passing through a given point, and let the pitch proper to this line be p ; the body will continue at rest under the action of P and a couple which has a moment $P p$ and acts in a plane at right angles to the line. Thus, using the same axes of coordinates as in equation (4), the body will be at rest under the action of any couple whose plane is parallel to

that of xy , since the pitch of the reciprocal screw in this case is infinite, and therefore P is zero.

We hope that the account we have now given of the author's Theory of Screws, so far as it goes, will be intelligible to readers unacquainted with the subject, and that it may induce some of them to peruse the work itself. Our limits will only allow us to add that in an Appendix of sixteen pages Mr. Ball gives an account of the memoirs bearing on the subject of his treatise.

LVIII. *Intelligence and Miscellaneous Articles.*

ON THE POLAR AURORAS. BY G. PLANTÉ.

"**N**OTHING should be neglected that can induce complete conviction, in the theory of the forces of nature, and enable us to pass from the disquiet of investigation to the security of known truth. Should we be quite sure, for example, of the theory of the rainbow, if we had not, by spiriting drops of water in the sunshine, reproduced in all its details that brilliant phenomenon? The experiments of the cabinet are modest, but useful, and therefore valuable." These words of an illustrious Member of the Academy encourage me to pursue the analogies between the effects of electric currents of high tension and the grand electrical manifestations of nature. De la Rive's experiment has already brought out the connexion of the polar auroras with terrestrial magnetism, but does not suffice to explain all the circumstances accompanying them. In the experiments which are the subject of the present memoir the electric flow takes place in the presence of aqueous masses, as in the atmosphere; and hence result a series of phenomena altogether similar to the various phases of the polar auroras.

1. If the positive electrode of the powerful secondary battery which I employ is put in contact with the moist sides of a vessel containing salt water in which the negative electrode is previously immersed, there is seen, according to the greater or less distance of the liquid, either a wreath round the electrode (fig. 1), or an arc bordered with a fringe of bright rays (fig. 2), or a sinuous line

Fig. 1.

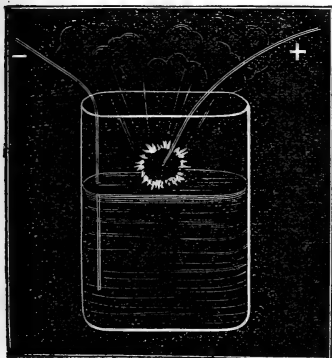
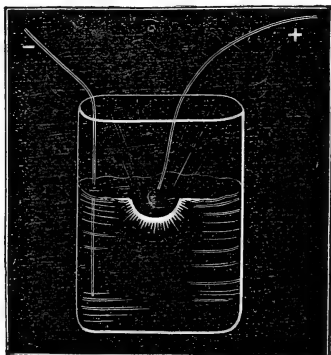
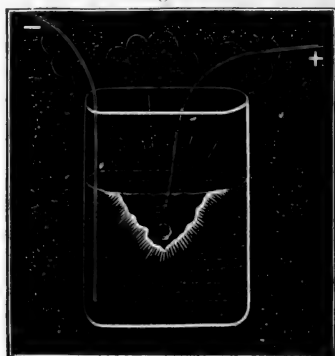


Fig. 2.



rapidly folding and refolding upon itself (fig. 3). This undulatory motion perfectly resembles that which, in the aurora, has been com-

Fig. 3.



pared to the bending and wriggling of a serpent or the undulations of drapery agitated by the wind.

2. Although, in consequence of the employment of salt water, yellow light predominates in these experiments, purple and violet tints similar to those of the aurora are also observed at the parts where the water from the condensed vapour is less charged with salt.

3. The rays of the luminous arc arise from the penetration of the electric flow into the liquid; and as the resulting vacuum is filled up, new rays are formed: thus we explain how it is that the jets of light of the aurora *dart*, or appear to be momentarily shot forth and renewed.

4. The dark circle or segment formed in the aurora by the mist or nebulous veil encountered by the electric flow corresponds to the humid circle or segment which, in the experiment, surrounds the electrode, and around which the voltaic current spreads.

5. The form of an arc in the voltameter results from the liquid not entirely surrounding the electrode; but if more of the wire be immersed, luminous waves or entire circles are produced, the same as in the auroras—of which the arc is often considered to be the portion visible to the observer, of a complete luminous circle.

6. The liquid is violently agitated by the electric blast; luminous vortices and rings are produced by the shock of the electrified waves against one another; and, finally, if the operation is conducted with only a small quantity of liquid, a luminous ebullition is produced corresponding to that fluctuation of light which also characterizes the polar auroras.

7. The deeper the electrode penetrates into the liquid, the more briskly and abundantly is aqueous vapour liberated. This phenomenon, of which the most powerful batteries of static electricity hardly afford a suspicion, is important for consideration, because

it explains naturally the abundant falls of rain or snow which have always been noticed during the polar auroras.

8. The sound that accompanies these experiments corresponds to that which has often been heard during the aurora when the distance was relatively small; it is due to the vaporization produced by the electric trails of fire penetrating a liquid mass.

9. The magnetic perturbations caused by the auroras are reproduced in these experiments, on placing a magnetized needle near the circuit. The deviation increases or diminishes as the luminous arc becomes more or less developed in the liquid.

10. It follows, further, from these facts that the aurora must be produced by a flow of *positive* electricity; for the luminous phenomena are the same as those at the positive electrode in the voltmeter, and the negative electrode exhibits nothing similar.

11. But are the polar auroras a discharge between the positive electricity of the atmosphere and that of the earth supposed negative? If they were so, we ought to observe very frequent falls of lightning at the poles, or gleams or luminous tufts on the projecting parts of the ground, forming the counterpart of the phenomenon that takes place in the air. Now observation shows that such is not the case. I am therefore inclined to think that it is the imperfect vacuum of the upper regions that, acting as an immense conductive envelope, plays the part of the negative electrode in the above experiments, and that the positive electricity flows off towards the planetary spaces, and not to the earth, through the icy mists or clouds which float above the poles.

12. As to the origin of this polar electricity, it has been assumed that it comes from the equator and the tropical regions. But it may be objected that the electrified clouds must discharge themselves during so long a journey; and, in fact, we know that tempests are rarer and rarer in proportion as we get nearer the poles. My previous experiments, and others not yet published, having led me to consider the heavenly bodies charged with positive electricity (perhaps the only sort that exists), I should be inclined to regard the earth itself as charged with positive electricity, which is liberated from the land and sea by simple emission, radiated from the whole surface, at the poles as at the equator, producing very different effects in the atmosphere in consequence of the diametrically opposite meteorological conditions of these regions.

Admitting this last hypothesis, one might conclude that the aurora results from the diffusion in the upper strata of the atmosphere, around the magnetic poles, of the positive electricity emanating from the polar regions themselves, either in obscure radiations when no obstacle is interposed, or converted into heat and light by meeting with aqueous masses in the solid or liquid state, which it vaporizes with a noise, and reprecipitates in the form of rain or snow at the surface of the globe.—*Comptes Rendus de l'Académie des Sciences*, vol. lxxxii. pp. 626–629.

ON STREAMS FROM GLACIERS. BY CHARLES KNIGHT, F.L.S.

To the Editors of the Philosophical Magazine and Journal,

GENTLEMEN,

Wellington March 7, 1876.

In reference to Dr. Pfaff's experiments on the plasticity of ice, is the following extract from my Address to the Philosophical Society of Wellington of sufficient interest to appear among the Miscellaneous Articles of the Magazine?

"I see in Geikie's late work, 'The Great Ice Age,' that reference is made to the fact that, from the foot of glaciers in Greenland, streams of water issue and unite to form considerable rivers, one of which, after a course of forty miles, enters the sea with a mouth nearly three quarters of a mile in breadth.

"This flow of water, Geikie thinks, probably circulates to some extent below every glacier; and he accounts for it by the liquefaction of ice from the warmth of the underlying soil. A more complete explanation of this flow of water from glaciers estimated not less than 3000 feet thick will be found in the suggestion first made by Professor James Thomson, and subsequently proved by his brother Professor W. Thomson, that the freezing-point of water is lowered $0^{\circ}23$ Fahr. for each additional atmosphere of pressure. Now a sheet of ice 3000 feet thick is equal to the pressure of about 80 atmospheres, at which pressure its temperature at the base should not exceed 13° Fahr. to retain the solid form. In the state of running water beneath the glacier, it might readily, as Geikie states, absorb heat from the underlying soil. In this we have a safe assurance that glaciers of such enormous thickness can exist only where there is scarcely any or no inclination of the land to the sea board. And we may hesitate to adopt with Geikie the views of the Swiss glacialists, who speak of sheets of ice having existed in the great Ice Age not less than 3000 feet thick, overtopping the Jura and stretching continuously from the Rhine valley.

"Sheets of ice 3000 or 5000 feet thick may exist, but not at the temperature of 32° Fahr. throughout the whole mass. The temperature at the surface may be 32° Fahr.; at the base it must be below 13° Fahr. The specific heat of water is far greater than that of ice. One pound weight of ice at 32° Fahr. mixed with one pound of boiling water gives 2 lbs. of water at 51° Fahr.; so that 71 degrees of heat are lost in the mere conversion of ice into water. Thus every pound of ice converted by pressure into water demands a large supply of caloric as a necessity of its change of condition, and absorbs it instantly from the ice above. The ice in immediate contact with the layer of water, hardened by loss of caloric, now robs the ice above of caloric, and again softened can no longer bear the pressure, and in its turn flows away as water; and so the process goes on, until a regular gradation of temperature is established throughout the mass, and an equilibrium formed between the forces by which the sheet of ice maintains a fixed altitude. No column of ice 3000 feet in height can maintain that elevation for an indefinite time, unless the temperature of the air is much lower than 32° Fahr., and the loss from liquification beneath constantly supplemented by renewed accumulations above."

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SUPPLEMENT TO VOL. I. FIFTH SERIES.

LIX. *The Formation of Meteorites, and Volcanic Agency.*
By Prof. G. TSCHERMAK, *Director of the Imperial Mineralogical Museum of Vienna* *.

THE researches of Howard, Klaproth, Vauquelin, and Berzelius on the elementary composition of many specimens of meteorites proved that the matter of which they are constituted is, generally speaking, such as occurs in greater abundance in the crust of our globe. Before then Chladni had established the planetary nature of these remarkable masses.

From these results taken together it may be assumed that the other heavenly bodies are built up of the same materials as those composing our globe. But this assumption has become a certainty, as regards the central body of our system, from the spectroscopic investigation of the light of the sun, initiated by Kirchhoff and Bunsen, while the observations of Secchi, Huggins, and Miller on the spectra of the fixed stars render it highly probable that the universe is formed of similar elementary materials.

Our conception of the material nature of the heavenly bodies is founded on the analysis of meteorites; and accordingly a study of the form of meteorites is likely to afford some insight as regards the antecedents of the star-masses whence they are derived, and the changes which they have undergone.

The external form of meteorites is remarkable in many respects. Hitherto but little attention has been directed to the very curious fact that meteorites reach us in the shape of fragments. It is well known that any one who has merely

* Translated from the *Sitzungsber. der k. Akademie der Wissenschaften*, vol. lxxi. p. 661, by Dr. Walter Flight, of the Mineral Department, British Museum.

read of the planetary nature of meteorites, experiences no little astonishment, on inspecting a collection of them, to find that they are not rounded as the planets are, but are angular, often exhibit sharp edges, and possess, even in their interior, no concentric structure.

Haidinger, who examined with much care the surfaces of meteorites, arrived at the conviction that their dark-coloured crust and the rounded nature of their edges were not original characteristics, but that the thin crust was developed during transit through the atmosphere, the sharpness of the edges being at the same time abraded. A meteorite, in short, before entering the atmosphere possesses an angular form; the greater number are acutely angular. The faces of the angular masses are surfaces of fracture; each meteorite is a fragment, and in fact owes its form to the disruption of a larger mass.

Any collection, in which complete meteorites are preserved, will furnish examples which conclusively demonstrate this fact; and among the specimens at Vienna attention may especially be directed to the meteoric iron of Agram, that of Ilimäë, and the stones of Knyahinya, Seres, Lancé, Chantonay, Orvinio, Tabor, Pultusk, Stannern, and others. The external form of these and other meteorites bears no relation whatever to the constitution of their interior; it is purely accidental.

It may perhaps be maintained that the disruption took place in the atmosphere; and there are unquestionably a few instances where an inspection of the crust shows that the meteorite had been sundered during its flight through the atmosphere; the fact, however, remains that meteorites enter our atmosphere in the form of fragments. After the aerolitic fall which took place near Butsura, India (May 12th, 1861), five fragments were discovered, some of them as much as six miles apart; by fitting these pieces together Maskelyne was able to reconstruct in London the meteorite in the form which it originally bore, and which appears to have been that of a comparatively thin slab somewhat bent. The irregular heating to which such a body would be subjected during its fall would lead to its disruption. With this example, it is scarcely necessary to direct attention to other cases which establish the fact that meteorites when they enter our atmosphere do not have the rounded form of planetary bodies.

Meteorites, then, always reach us in the shape of fragments, as flattened masses, or as fine particles, which have their origin in one or more larger planetary masses. Whether the source be one or many, the dimension of this mass or these masses must have been not inconsiderable.

The greater number of meteoric irons exhibit a structure which indicates that each has formed part of a large mass possessing similar crystalline characters. The formation of large masses so constituted, presupposes, as Haidinger has pointed out, long intervals of time for tranquil crystallization at a uniform temperature; and these conditions could only prevail on one of the larger cosmical masses. On the surfaces of many meteorites (such as the Château-Renard, Pultusk, and Alessandria ones) indications resembling in every particular the "slide" or "throw" of terrestrial rocks are to be met with, appearances which indicate the disruption or dislocation of larger masses. Other meteorites, like those of Tula, Chantonay, Orvinio and Weston, are congeries of angular fragments, and correspond, as regards structure, with terrestrial breccias. Many stones, moreover, are formed of very small fragments, of tiny splinters, and resemble volcanic tuffs. These characters, again, point to larger cosmical masses which have undergone mechanical changes as the source whence they have been derived.

So far, then, it would appear that the material of which meteorites consist has been furnished by one or more large masses, the formation of which must have occupied a long period of time. The question which therefore suggests itself is, How is the process of disintegration to be accounted for? This Daubrée endeavoured to solve, and arrived at the alternative that disintegration must be the effect either of impact or explosion*.

The idea that the smaller planetary bodies were formed by the impact and disruption of larger cosmical masses was propounded by Olbers† to account for the existence of the asteroids; and D'Arrest and C. von Littrow have in more recent times established, by careful calculation, the possibility of the impact of these bodies actually taking place.

By the impact of two solid cosmical masses, moving with planetary velocity, fusion, perhaps even vaporization, would occur over the area of contact‡; disintegration, moreover,

* G. A. Daubrée, *Journal des Savants*, 1870.—Meunier (*Géologie Comparée*, 296) seeks to escape from this dilemma by supposing a spontaneous disruption of the planetary mass to have taken place in much the same way as a disk of clay crumbles on drying. Even if we allow this explanation to be a possible one, the further difficulty presents itself, that the fragments would continue to traverse one and the same orbit, which, with the meteorites, we know is not the case.

† Zach, *Monatl. Correspondenz*, vol. vi. p. 88.

‡ A cosmical mass possessing a velocity of three geographical miles, and by impact with another mass being brought to a state of rest, would, on the supposition that all the *vis viva* was converted into heat and none

would be a result; the fragments would be sundered with great violence, and would acquire velocities in several directions. The formation of meteorites may thus be explained; we must, however, not lose sight of the fact that during such a disruption large fragments, as well as small, would be produced. Meteorites, however, are always small masses. The largest known are the meteoric stone of Knyahinya, in the Vienna Collection, which weighs 294 kilog., and the meteoric iron of Cranbourne, preserved in the British Museum, and weighing 3700 kilog. Most of them are of far less magnitude; and a stone of 5 kilog. is accounted a large meteorite.

All these masses, even the largest, are but tiny splinters or fine dust in comparison with a very small planet of only a mile diameter. Were the latter divided into a million equal parts, one of them would be 250,000 times the size of the Knyahinya stone, and 10,000 times as large as the Cranbourne meteoric iron.

It appears, therefore, to be highly improbable that meteorites owe their form to the disruption, by impact, of planetary masses; it is much more probable that a disintegration, even to the smallest particles (what may be termed a pulverization), has been brought about by a force acting from within outwards—in short, by an explosion.

Explosion is a violent process, and may appear to clash with our ideas of gradual cosmical development; yet it is not more violent than the movements observed on the surface of the sun, or those of the comets, whether observed or calculated. The explosive upheavals on the sun observed by Zöllner, Young, and Respighi, the cyclonic movements, as determined by Lockyer, proceed at far greater velocities than have ever been observed in the case of terrestrial explosions.

Again, the sudden blazing-forth of certain stars is evidence of the activity of some vast force, which Mayer traced to the impact of fixed stars and their union after fusion. The emanations observed in comets by Schmidt take place with a vigour which also implies very rapid movement. There is nothing in any of these phenomena at all at variance with the assumption of an explosion or pulverization of a cosmical body. Whether the cosmical mass or masses which have furnished the meteorites are to be ranked with the fixed stars,

of the heat dissipated, develop for each unit of weight 59630 units of heat. If we assume one half the heat to be lost by radiation and conduction, and the specific heat of a meteoric stone to be five times as great as it is (that is to say, as = 1), in order to compensate for the increase of specific heat with the temperature and for the heat of fusion, the rise of temperature would still be = 29800° C.

the planets, or the comets, the inference which we need not hesitate to draw in either case is, that the masses in question have been disintegrated by explosion.

Here, however, we have to meet the same objection as in the theory of disintegration by impact. When a cosmical body of considerable magnitude, whether solid or in part fluid, is sundered by explosion, there will be produced, in addition to the numberless small fragments, large masses also, which become meteorites, and set out in various orbits. We must not, however, lose sight of the fact that meteorites, as we know them, are, comparatively speaking, very small bodies ; and for this reason we cannot support the view that the complete breaking asunder of the mass had occurred at a single explosion. But the disintegration of such a cosmical mass may be gradual. In place of a single explosion, we can conceive a succession of them hurling portions of the surface into space. Such an occurrence may take place on any cosmical mass on which volcanic agencies may be active, and the mass yet be so small that its gravitating power is insufficient to attract again to its surface all the fragments which have been hurled from it.

Such considerations recall to mind the views long since propounded by Olbers, Arago, Laplace, Berzelius, and others, and recently put forth by Dr. Lawrence Smith, according to which the moon, the gravitating force of which is one sixth that of the earth, could propel fragments to such distances that they would not return to her. The possibility of such an occurrence in the case of the moon cannot be gainsaid ; the lunar surface covered with its numerous crater-mounds shows that the greater portion of the ejected masses have descended again to form the stony ramparts surrounding the crater-mouths, and that even under the most favourable conditions only few fragments have been scattered into space. When the amount of meteoric matter which reaches our earth annually is considered, such a source as the one in question appears to be too insignificant. The meteorites reach us from such a variety of directions, and fall so frequently, that we must seek for a more general cause, lying not in the moon alone, nor, in short, in any single member of the system of heavenly bodies.

In a number of cosmical bodies, then, of considerable dimension, yet sufficiently small to be unable to attract again to their surface fragments hurled from them by explosion, it is possible to conceive the conditions under which meteorites are fashioned. It is highly probable that small star-masses of such a kind may, during a certain period of their

existence, exhibit violent explosive activity, if we may judge by analogy from the moon, which has passed through a stage of much more intense volcanic activity than we have any experience of. Such smaller star-masses continue to lose in mass by constant projection of fragments, until at last they themselves are resolved into small portions, and traverse the universe in orbits of the most varied kind.

We might feel inclined to recognize in the comets the débris of such small star-masses, and to detect in their emanations the last phase of that activity which we have described. That, however, is beyond our province; and it must be left to those investigators who deal with the question of the nature of comets to decide whether the observations already made are calculated to establish such a connexion*.

It will suffice here to have shown that the form of meteorites may be assumed to be due to violent movements of the original star-mass acting from the interior towards the surface. Similar movements are at present taking place on the earth and on the sun, while at an earlier period it was by such movements that the craters of the lunar surface were built up. On different star-masses the cause of these movements may be different; and so long as the cause is in any instance unknown it is permissible to term all these movements volcanic.

Whether the action be simply explosive, hurling upward the solid rock of the surface, or at the same time also eruptive, as on the earth, ejecting matter from the interior, in either case a difference will be observed between the crust and the nucleus of the rock. As meteorites reach us in the form of angular fragments, it follows that the star-mass whence they are derived must have possessed a solid crust; further, we must conclude that the interior was either not solid, or had an altogether different constitution.

Guided by their form we might trace the origin of meteorites to smaller star-masses, constituted similarly to our earth, which by volcanic activity have been gradually reduced to fragments. The texture of the meteoric rock carries us a step further towards obtaining a glimpse at the history of the star-mass before its disintegration.

It has already been stated that the constitution of many of

* There are many who trace a connexion between the meteorites and the shooting-stars because the atmospheric phenomena in each case are almost entirely the same. As the connexion between the comets and the shooting-stars had been discovered and explained by Schiaparelli, a relation between the comets and the meteorites followed as a matter of course. A difficulty, however, is presented by the fact that the maxima of frequency of star-showers in no way accord with the most abundant aërolitic falls.

the meteorites shows that they are the result of a gradual tranquil crystallization, while others, on the contrary, are composed of fragments, and are the product of disintegrating forces. The majority are made up of minute flakes and splinters and of rounded granules.

Haidinger was the first who ventured to compare the porous masses, made up of rock-dust, with the triturated and pulverized products of terrestrial volcanoes, and to term them meteoric tuffs. The marked preponderance of this structural character in meteorites shows that in the star-masses whence they were derived a state of quiescence must have been of much rarer occurrence than one of volcanic activity.

In the tufaceous meteorites a characteristic feature presents itself which throws considerable difficulties in the way of our explanation, a phenomenon which is not observed in so marked a degree in the tuffs of our volcanoes. It is the occurrence in great abundance of minute rounded particles and spherules which at once arrest the attention of the observer. They characterize all tufaceous meteoric stones which, as already stated, constitute the largest class of these bodies, and which were termed by Rose "chondritic."

These spherules present the following features, which suffice to enable us to recognize their mode of formation* :—

1. They are imbedded in a matrix consisting of fine or coarse splinter-like particles.

2. They are invariably larger than these particles.

3. They are always distinct individuals, never merging into each other or joined together.

4. They are quite spherular when composed of a tough mineral, and in other cases merely rounded in form.

5. They consist sometimes of one mineral, sometimes of several minerals, but always of the same material as the matrix.

6. The structure of the interior of a spherule is in no way related to its external form. They are either fragments of a crystal, or have fibrous structure (the fibres taking an oblique direction towards the surface), or have irregularly barred structure, or they are granular.

These chondra bear no indications of having obtained their spherular form by crystallization, and cannot be classed with spherulitic obsidian or perlite, the spherules of orbicular diorite, or the rounded concretions of calcite, aragonite, or marcasite.

* Drawings of these spherules are given in the papers by Tschermak on the Gopalpur meteorite (*Sitzungsb. Ak. Wiss. Wien*, lxx. 122) and on the Orvinio meteorite (*Sitzungsb. Ak. Wiss. Wien*, lxx. Abth. i.), and that by Von Drasche on the Lancé stone (*Mineralogische Mitt.* 1875, i.).

They resemble rather the spherules which are frequently met with in our volcanic tuffs, such as the chondra of the trachyte tuff of Gleichenberg, those of the basalt tuff of the Venusberg, near Freudenthal, and more especially the spherules of olivine of Kapfenstein and Feldbach in Styria.

As regards the last-mentioned chondra*, we know them to be the result of volcanic trituration, and to owe their form to a prolonged explosive activity in a volcanic "throat," where the older rocks have been broken up and the tougher particles have been rounded by continued attrition.

The characters of the meteoric chondra indicate throughout a similar mode of formation†. We may at any rate conceive the rock-masses which suffered trituration to have possessed a somewhat soft texture, and thus arrive at an approximation to Daubrée's view‡ of a rock-mass which assumed a solid form while rotating in a gaseous medium; it is certain, in short, that the spherules are the result of trituration.

The spherules are sometimes microscopically small, have sometimes the size of a millet-seed; others may be as large as a cherry or a small hazel-nut; but they are not numerous. The tufaceous chondra of our volcanic rocks vary in size from that of a hazel-nut to that of a head. If from this disparity we may form any conception of the different magnitudes of the areas of activity in which they have been produced, we must seek in numerous minute volcanic fissures for the source of the meteoric tuffs.

The latter are peculiarly characterized as containing no trace of a slag-like or vitreous rock, nor enclosing distinct crystals in the matrix; in short they exhibit nothing which their formation from lava would lead us to look for. All that is to be seen in them is the triturerated product of a crystalline rock.

Some of the tufaceous meteorites bear evidence of a later modification wrought by heat: such are the stones of Tadjera and Belgorod§. Others, again, exhibit phenomena which can only be explained on the theory of their having undergone a chemical change subsequent to their formation. We not unfrequently find, for example, in the meteorites of Mezö-Madaras and Knyahinya concentric aggregations of metallic

* They must not be confounded with the volcanic bombs which are composed of lava.

† Reichenbach considered the chondra to be small meteorites. Here, however, the aim is merely to form a conception of a planetary form of meteorites.

‡ G. A. Daubrée, *Journal des Savants*, 1870, 38.

§ *Sitzungsb. Akad. Wiss. Wien*, vol. lxx. Abth. i. November Heft.; and S. Meunier, *Compt. Rend.* vol. lxxii. p. 339.

iron enclosing the chondra, which in a microscopic section of the stone present much the same appearance as the halo surrounding the lunar disk; similar appearances are likewise to be often observed in the spherule itself. All tufaceous meteorites have, disseminated throughout their mass, innumerable minute spangles of nickel-iron. These phenomena premise the reducing action of a gas; and Daubrée assumes that these changes have been wrought by hydrogen. The discovery of hydrogen in the Lenartó meteoric iron by Graham, and the fact of the presence of hydrogen in the sun, established by Kirchhoff, support this view. An exposure to high temperatures in such a case is, of course, likewise assumed.

Distinct evidence of such exposure is, moreover, to be met with in the case of meteorites which consist of fragments cemented together with material of similar composition to those fragments, as in the case of the stones of Orvinio and Chantonay*. Still, with the many proofs which we possess of the action of heat, we have not yet met with a meteorite which resembles a volcanic slag or a lava. Although the meteorites are comparable to volcanic tuffs and breccias, this comparison cannot be extended beyond a certain point. The volcanic activity, of which the meteorites furnish evidence, consisted in the disintegration of solid rock, in the modification, by heat and otherwise, of already solidified masses. Outflows of lava, and ejection of lava-glass and crystals, which, as Zirkel has shown, form volcanic ash, have not taken place in the case in question.

It is, then, by explosive activity, and that alone, that the breccias and tuffs which we find in meteorites have been formed. We are here forcibly reminded of a similar terrestrial phenomenon with which most are familiar, since in the "Maars" of the Eifel we have what must rightly be regarded as craters of explosion, and a proof that our earth also can furnish an instance of volcanic explosive activity without eruption of lava.

The question which still has to be considered is, What is the cause of that volcanic activity which has subjected the rock forming the surface of such a star-mass to disintegration and trituration, and by which the entire mass of a heavenly body may gradually be reduced to fragments?

The question does not limit itself to what we have dwelt on, but embraces a general consideration of cosmical volcanic agency. Both on the sun and on our earth it is by means of gases and vapours that volcanic energy is transmitted. On the moon we find no traces of an atmosphere, which it would

* *Sitzber. Akad. Wiss. Wien*, vol. lxx. Abth. i. November Heft.

probably have possessed if the lunar craters had been formed by gaseous explosion. In view of this difficulty a suggestion has been put forth in a recently published work*, that the volcanic activity on the lunar surface has been due to expansion attending solidification. Were this a correct interpretation, it would equally follow that during the freezing of water, which also expands on becoming solid, eruptive phenomena, accompanied by the formation of craters, would certainly be occasionally observed; but such, it need hardly be stated, are unknown. It appears to us, however, that the difficulty which would be disposed of by such an hypothesis has never actually been presented. It cannot have been by means of permanent gases alone that the volcanic changes on the moon have been brought about; and if vapours took part in these operations they must have been subsequently absorbed by the rocks forming the lunar surface. We have, however, thus far not been driven to have recourse to Sämann's theory†, according to which the surface of the moon was at an earlier epoch covered with water which has subsequently been absorbed. We shall defer the examination of this subject to a later occasion.

All these considerations, then, have led us to the conclusion that volcanic agency, resulting in the disintegration and distribution of rocky masses in space, is inconceivable without the simultaneous action of gases or vapours, or of perhaps both. From this it follows as a direct inference that the explosive activity, to the existence of which meteorites bear evidence, is due to the sudden expansion of gases or vapours, among which hydrogen may have been a prominent agent.

The conclusions arrived at from the careful investigation and comparison of meteorites, confirm the results obtained during the last few years by students of geology and astronomical physics. The volcanic activity of which those mysterious masses of stone and metal are evidence may be compared to the violent movements on the solar surface, the more feeble action of our terrestrial volcanoes, or the stupendous eruptive phenomena of which the lunar craters tell the history.

No one who at this juncture bears in mind Kant's theory of the homogeneous development of stellar masses, can fail to be struck with the idea that the heavenly bodies already alluded to are not the only masses which have undergone these changes, but will rather incline to the opinion that volcanic activity is a cosmical phenomenon in the sense that

* Nasmyth and Carpenter, 'The Moon' (London, 1874), p. 98.

† Sämann, *Bull. Soc. Géol.* [2] xviii, 322.

all star-masses at a stage of their development exhibit a phase of volcanic activity. And it is possible that during this period many of those masses which are of comparatively trifling magnitude may have been partially or wholly broken up and resolved into small fragments.

LX. *On the Nebular Hypothesis.—II. Interaction.* By PLINY EARLE CHASE, *Professor of Philosophy in Haverford College.*

[Continued from p. 319.]

THE tendency of vibrations in elastic media to produce harmonic vibrations, combined with the tendency to geometrical and harmonic as well as to arithmetical progressions, should prepare us to look for relations of various complexity when we study the mutual actions and reactions of planetary masses.

The triangular numbers, 1, 1 + 2, 1 + 2 + 3, 1 + 2 + 3 + 4, appear as indices in the following equation among powers of the masses of the five principal bodies in the solar system :—

$$\left(\frac{\Psi}{\odot}\right)^1 \times \left(\frac{\hat{\odot}}{\odot}\right)^3 \times \left(\frac{\mathcal{U}}{\odot}\right)^6 = \left(\frac{h}{\odot}\right)^{10}.$$

This appears to be the first equation ever discovered which introduces the products of powers of masses in so systematic a form*. Although its full interpretation may be at present beyond our reach, we can catch glimpses of its meaning, and we may feel a reasonable assurance that it represents some important functional law of equilibrating tendencies between centripetal and centrifugal forces. The truth of the equation is, of course, independent of any assumption with regard to the proper unit of comparison; but the dominance of the solar influence lends interest to the æsthetic harmony afforded by its introduction. This interest is increased by the accordant between the order of position and the magnitude of the indices in the left-hand member of the equation, and by the fact that the nebular centre of planetary inertia ($\sqrt{\Sigma mr^2 \div \Sigma mr}$) is in Saturn's orbit.

There is still some uncertainty as to the masses of Neptune and Uranus; so that it is impossible to tell how close this agreement may be; but the deviation from precise accuracy cannot be large. According to Newcomb's latest determinations of

* Laplace, however (*Méc. Cél.* II. vol. viii. pp. 65–69; VI. vol. ii. pp. 12–16, &c.), investigated inequalities depending on squares and products of the disturbing forces. In his discussions of the Jovian and Saturnian systems he introduced terms containing the third and fifth dimensions of eccentricities and inclinations.

those masses*, from Neptune's satellite and from perturbations of Uranus, the equation gives two values for Saturn's mass, one of which is slightly larger, the other slightly smaller than Bessel's value. There are, however, other relations of a similar character which can be measured with great accuracy.

Laplace found that if the mass of each planet be multiplied by the product of the square of the eccentricity and the square root of the mean distance, the sum of all the products will always retain the same magnitude; also that if each of the masses be multiplied by the product of the square of the orbital inclination and the square root of the mean distance, the sum of the products will always remain invariable. Now the square root of the mean distance varies inversely as the velocity of circular revolution at the mean distance, or inversely as the square root of the velocity of nucleal rotation at the same distance. It is therefore probable that the primitive nucleal undulations, to which I have hypothetically attributed both the relative positions and the relative masses of the planetary orbs, may have left their record in many other directions than those which I have already pointed out.

Stockwell has found † the following relations:—

“I. *The mean motion of Jupiter's perihelion is exactly equal to the mean motion of the perihelion of Uranus; and the mean longitudes of those perihelia differ by exactly 180°.*

“II. *The mean motion of Jupiter's node on the invariable plane is exactly equal to that of Saturn; and the mean longitudes of these nodes differ by exactly 180°.*”

If the four great planets were ranged in a line, Jupiter on one side of the sun and the remoter planets on the other, the tidal influences, relatively to the nucleal centre of inertia, would drive Jupiter, Uranus, and Neptune to their respective aphelia. Those positions would accord with Stockwell's two theorems, they would approximate the planetary centre of inertia very closely to Saturn's mean radius vector, and they would make the above equation of the products of triangular powers applicable to vector radii as well as to masses; for the logarithms of mean vector radii and of their designated triangular powers, according to Stockwell ‡, are:—

Neptune, mean aphelion ...	1.481951	(Neptune) ¹	1.481951
Uranus, “ “ ...	1.301989	(Uranus) ³	3.905967
Jupiter, “ “734588	(Jupiter) ⁶	4.407528
			10)9.795446
Saturn, mean979496	Saturn	.979545

* “The Uranian and Neptunian Systems,” Washington Obs. for 1873, App. I.

† Smithsonian Contributions, 232, p. xiv.

‡ Ibid. p. 5.

The difference between the actual value of $\log h$ *r. vec.* and the value as found by the equation $\Psi^1 \times \mathfrak{S}^3 \times \mathcal{U}^6 = h^{10}$, is therefore only .000049, representing a numerical difference of only $\frac{1}{80}$ of 1 per cent.

When the hypothetical nebular condensation had proceeded so far as to show the controlling planetary influence of Jupiter's mass, the mean perihelia of Saturn and Uranus were so fixed as to establish the following relationships of harmonic powers (mean perihelion, mean, and mean aphelion* being designated by the subscript figures 1, 2, 3 respectively).

Stockwell's logarithmic values are :—

Neptune ...	Ψ_3	1.481951 α	Ψ_1	1.473327 α'
Uranus ...	\mathfrak{S}_1	1.262996 β	\mathfrak{S}_3	1.301989 β'
Saturn ...	h_1	.957973 γ	h_2	.979496 γ'
Jupiter ...	\mathcal{U}_3	.734588 δ	\mathcal{U}_2	.716237 δ'

$$\frac{1}{3}(\alpha - \beta) + \frac{1}{2}(\beta' - \gamma) + (\delta - \gamma') = .000085 = \log 1.0002$$

$$\frac{1}{3}(\alpha' - \beta) + \frac{1}{2}(\beta' - \gamma) + (\delta' - \gamma) = .000382 = \log 1.0009$$

These results represent the following equations of distance :—

$$\left(\frac{\Psi_3}{\mathfrak{S}_1}\right)^{\frac{1}{3}} \times \left(\frac{\mathfrak{S}_3}{h_1}\right)^{\frac{1}{2}} \times \left(\frac{\mathcal{U}_3}{h_2}\right)^{\frac{1}{6}} = 1; \quad \left(\frac{\Psi_2}{\mathfrak{S}_1}\right)^{\frac{1}{3}} \times \left(\frac{\Psi_3}{h_1}\right)^{\frac{1}{2}} \times \left(\frac{\mathcal{U}_2}{h_1}\right)^{\frac{1}{6}} = 1.$$

The theoretical differ from the actual values by less than $\frac{1}{50}$ of 1 per cent. in the first, and by less than $\frac{1}{11}$ of 1 per cent. in the second equation. The closeness of these agreements may, perhaps, induce a glance at a few that are somewhat more intricate.

If we substitute for the theoretical primitive figurate exponents (1, 3, 6, 10) the present actual vector radii ($a = \Psi_2$; $b = \mathfrak{S}_2$; $c = h_2$; $d = \mathcal{U}_2$), we may form an equation for Saturn's mean perihelion :—

$$\Psi_2^a \times \mathfrak{S}_2^{b-d} \times \mathcal{U}_2 = h_1^{a+b}. \quad . \quad . \quad . \quad . \quad . \quad (1)$$

If a, b, d represent the mean *aphelion* vector radii, the equation represents Saturn's mean distance :—

$$\Psi_3^d \times \mathfrak{S}_3^{b-d} \times \mathcal{U}_3^a = h_1^{a+b}. \quad . \quad . \quad . \quad . \quad . \quad (2)$$

If we take powers of the masses instead of powers of the vector radii, equation (2) yields two values for Saturn's mass, according as we use Newcomb's greatest or least value of Neptune's mass—

$$\text{the greatest } (\frac{1}{19380}), \text{ deduced from } \Psi \text{'s satellite,} \quad . \quad . \quad (3)$$

$$\text{or the least } (\frac{1}{19700}), \quad , \quad , \quad \text{perturbations of } \mathfrak{S}. \quad (4)$$

These four equations give approximations to precise accu-

* Smithsonian Contributions, 232, p. 38.

racy, varying between $\frac{3}{5}$ and $\frac{1}{20}$ of 1 per cent., as will be seen by the following solutions :—

(1)

$$\begin{array}{rcl}
 \log 30.03386^{5.202798} & . & 7.687712 \\
 ,, 19.183581^{19.183581-5.202798} & . & 17.936362 \\
 ,, 5.202798^{30.03386} & . & 21.511361 \\
 & & \hline
 & & 47.135435 \\
 ,, 9.077645^{30.03386+19.183581} & . & 47.148979 \\
 (47.148979 - 47.135435) \div (30.03386 + 19.183581) & & \\
 = \log 1.00051. & &
 \end{array}$$

(2)

$$\begin{array}{rcl}
 \log 30.33551^{5.427351} & . & 8.043068 \\
 ,, 20.044183^{20.044183-5.427351} & . & 19.030955 \\
 ,, 5.427351^{30.33551} & . & 22.284102 \\
 & & \hline
 & & 49.358125 \\
 ,, 9.538852^{30.33551+20.044183} & . & 49.346714 \\
 (49.358125 - 49.346714) \div (30.33551 + 20.044183) & & \\
 = \log 1.0005. & &
 \end{array}$$

(3), (4)

If $10 = \log$ of sun's mass, the logarithms of the assumed mass are :—

$$\begin{array}{rcl}
 \Psi \text{ (Newcomb, from satellite)} & . & 5.712646 \\
 \Psi \text{ (" " perturbations)} & . & 5.705534 \\
 \delta \text{ (")} & . & 5.645892 \\
 h \text{ (Bessel)} & . & 6.455734 \\
 \gamma \text{ (")} & . & 6.979689
 \end{array}$$

Substituting these logarithms in equation (2), we get for logarithm of Saturn's mass, by using

$$\begin{array}{rcl}
 \text{Satellite value of } \log \Psi & . & (3) \quad 6.458198 \\
 \text{Perturbation value of } \log \Psi & . & (4) \quad 6.456439 \\
 6.458198 - 6.455734 & = & .002464 = \log 1.0057 \\
 6.456439 - 6.455734 & = & .000705 = \log 1.0016.
 \end{array}$$

These relations are suggestive of the many familiar equations between the *sums* of periodic times. The substitution of *products* for sums, and powers for products, in the distribution of mass and position, seems to indicate the early organizing activity of constant forces, acting with reference to nucleal centres, in elastic media.

Philadelphia, March 30, 1876.

LXI. *On the Simultaneous Sounding of two Notes.*
By Dr. RUDOLPH KÖNIG, Paris.

[Continued from p. 446.]

III. *Difference-notes and Summation-notes.*

IT is well known that Helmholtz has proved theoretically that "whenever the vibration of the air, or of any other elastic body set in motion by both the primary notes at the same moment, becomes so strong that the vibrations can no longer be considered as infinitesimal, vibrations of the air must arise whose pitches are equal to the difference and to the sum of the number of vibrations of the primary notes." These combination-notes, both those of the difference and those of the sum, are quite distinct from the beats, and are much weaker than the original note.

If we turn our attention first to the difference-notes, we find that, in all intervals $n : n + m$, when m is not much greater than $\frac{n}{2}$ they coincide with the original note, and therefore cannot be proved by it. We have seen, however, that, in all intervals $n : n + m$, when m is much greater than $\frac{n}{2}$ the beat-notes are $m' = n - m$. In the intervals $n : hn + m$, when m is less than $\frac{n}{2}$, $m' = m$; and when m is much greater than $\frac{n}{2}$, $m' = n - m$, and therefore not the same as the difference of the vibrations of the primary notes. We must therefore try to observe the difference-notes in these intervals.

As I have already stated, these intervals, composed of high notes, allow the beat-notes to be heard quite loud, while no trace of the difference-notes is to be perceived. $c''' : b'''$ (8 : 15) allows only c (1) and no trace of 7 to be heard; $c''' : d^{iv}$ (4 : 9) only c' (1) and nothing of e''' (5); $c''' : f^{iv}$ (3 : 8) only f' and f'' , and absolutely no a''' (5); and it follows, therefore, that the difference-notes in any case must be immeasurably weaker than the beat-notes. I was able, however, to prove their existence beyond a doubt by forming the above quoted intervals from deeper notes, which, by their longer duration, enabled me to make use of auxiliary tuning-forks which gave a certain number of beats with the required notes.

If I allowed the great forks c' and b' (8 : 15) to sound in front of the sounding-boxes, the first thing that fell on the ear was the loud rattle of the 32 beats $m' = n - m$; if, however, I held a tuning-fork of 440 v. s. at a greater distance

from the ear, the four beats with the note $7=448$ v. s. were audible. In the same way I was able, by the clang of the notes c' and d'' ($4:9$), to prove through the beats the existence of the very soft note e' (5) with the help of a tuning-fork of 648 v. s., and by the clang of the notes c' and f'' ($3:8$) that of a soft a' (5), with a fork of 860 v. s.

As regards the observation of the summation-notes, Helmholtz has remarked that "these are only to be heard under peculiarly favourable circumstances—for instance, on the harmonium and on the many-voiced siren (*Tonempfind.* vol. iii. p. 244). But even if it is really sometimes possible, on sounding simultaneously two clangs on a siren or on a reed instrument, to distinguish notes of which the pitch is equal to the sum of the primary fundamental notes of both sounds, still this is not sufficient to prove the existence of the summation-notes, as neither sirens nor reed instruments produce simple notes, but sounds which are rich in overtones; and a slight examination shows that in consequence of this the mere beat-notes which must be produced by the overtones are sufficient to prove the existence of notes whose vibrations are equal to the sum of the vibrations of the fundamental notes of these sounds.

Two sounds in the interval of a fifth contain these two series of notes,

$$\begin{array}{ccccccc} 2, & 4, & 6, & 8, & 10, \\ 3, & 6, & 9, & 12, & 15; \end{array}$$

and the fifth notes of both sounds (10 and 15) produce a beat-note $m=m'=5$, which is equal to the sum $2+3$ of the roots.

In the fourth, $3:4$, we have the two series of notes,

$$\begin{array}{ccccccc} 3, & 6, & 9, & 12, & 15, & 18, & 21, \\ 4, & 8, & 12, & 16, & 20, & 24, & 28; \end{array}$$

and it is here the seventh notes of these sounds which produce a beat-note that is equal to the sum $3+4$. In the third, $4:5$, we have the overtones 36 and 45 , from which a beat-note must ensue which will equal the sum $4+5$; and thus in every ratio of the form $n:n+1$ the beat-note of the $2n+1$ st notes of both clangs is equal to the sum of the fundamental notes.

In intervals of the form $n:n+2$ there are also two notes of the same order, namely the $n+1$ st of both clangs, whose beat-note is equal to the sum of the fundamental notes. Thus the sixth, $3:5$, gives the notes

$$\begin{array}{cccc} 3, & 6, & 9, & 12, \\ 5, & 10, & 15, & 20, \end{array}$$

where the beat-note m is produced by 12 and $20=8=5+3$.

Lastly, in intervals of the form $n:n+3$ there are notes of

dissimilar order, namely the $n + 2$ nd note of the deeper clang, and the $n + 1$ st of the higher one, whose beat-note is equal to the sum of the fundamental notes. Thus, for example, in the minor sixth, $5 : 8$, the seventh note of 5 (35) and the sixth of 8 (48) gave the beat-note m , which is equal to the sum $5 + 8$.

It might perhaps appear strange that we should have remarked especially the beat-notes of overtones of two sounds whose vibrations were equal to the sums of both fundamental notes, while the beat-notes of many other overtones must also be audible; but it must be remarked that the number of these notes which are audible is by no means so great as we might be disposed to assume without a closer inspection. Thus the overtones of a fifth interval, up to the fifth, allow no beat-note, excepting the note 5, to be heard higher than the fundamental note, which does not coincide with one of the overtones of the two clangs. In the fourth, except the note 7, only the beat-note 5 of the first seven overtones arising from 15 and 20 does not coincide with the notes which are already contained in the clangs; and the ratio is the same in the other intervals.

The beat-notes in all the above-mentioned cases are equal to the difference of the notes by which they were formed, and therefore coincide with the difference-notes of these same notes; if, however, we take into consideration the great intensity that two primary notes must have in order to produce only a very weak difference-note, we may assume with tolerable certainty that the intensity of the difference-notes produced by the overtones must be very much less than that of the beat-notes with which they coincide.

It is further to be observed that on the siren and the harmonium not only are the singly produced notes accompanied by overtones, but also, if two sounds are produced at the same time, neither of them can be perceived except as produced by a series of similar impulses succeeding one another; for if the openings of two concentric circles of the siren are opened at the same time, the intensity of the impulse is not double as great as it would be if only one circle of holes were opened; and this diminution of intensity of the impulses at the moment of their coexistence, which is only produced by the disposition of the instruments employed, is sufficient alone to produce phenomena which have nothing to do with those of the simultaneous sounding of simple notes produced by isolated sound-waves (*Tonempfind.* vol. iii. p. 627; Terquem, *Annales de l'École Normale*, vii. 1870). If, therefore, we wish to be sure that we really have to do with combination-notes of simple primary notes, we must set aside both the many-voiced siren and the reed-pipes, and only make use of simple tuning-forks.

Tuning-forks for the notes c' , e' , g' , c'' , with prongs 6 milims. thick, upon sounding-boxes, as they are generally used in physical experiments, notwithstanding their somewhat considerable intensity, form only such weak summation-notes that auxiliary forks which give beats with them are necessary to prove their existence beyond doubt. If we possess a series of tuning-forks for the harmonic notes of the fundamental note c , the intervals $c' : g'$ and $g' : c''$ are specially adapted for the proof of the summation-notes by means of the beats, as the auxiliary forks for these can be easily arranged by untuning, by means of a little wax, the forks of the series mentioned for e'' and for the seventh upper note of c . But with such strong notes as I have made use of, the summation-notes are in themselves sufficiently loud to be perceived without auxiliary forks. At $c' : g'$ (2 : 3) we can distinctly hear e'' (5), which with c' and g' again forms the summation-notes of the second order 7 and 8 (c'''), which makes itself known by beats with the suitable auxiliary forks; and other auxiliary forks allow even the summation-notes of the third order $2 + 7 = 9$ (d'''), $2 + 8$ and $3 + 7$ (e'''), and $3 + 8 = 11$ to be noticed, though only by very faint beats. In the same way we hear also at $c' : e'$ (4 : 5) the note $9 = d''$, and by means of the auxiliary forks can prove the notes $9 + 4 = 13$, $9 + 5 = 14$, and the summation-notes of the third order 17, 18, and 19. Intervals with the fundamental note $c' = 512$ v. s. are generally the best suited for the observation of the difference- and summation-notes, as in these, on the one hand, the rattle of the discontinuous beats causes little if any disturbance, and, on the other hand, the beat-notes, on account of their great depth, have only a very slight intensity.

From the observations here given it follows, therefore, that difference-notes and summation-notes can be demonstrated by the simultaneous sounding of simple notes produced from separate sources of sound if these have a very great intensity, but that they are extraordinarily weaker than the beat-notes; so that at the simultaneous sounding of two clangs with tolerably powerful overtones, in most cases the audible notes, whose vibrations are equal to the sum of the primary notes, will in all probability be beat-notes of the upper notes, and not summation-notes of the primary notes.

These combination-notes are as little reinforced by sounding-boxes as the above-described beat-notes.

IV. *Upon the nature of Beats, and their operation compared with the operation of Primary Impulses.*

As the number of the vibrations of the summation-notes does not agree with the number of the beats of the two pri-

mary notes, and cannot, therefore, have been produced by them, this circumstance is amongst the grounds which Helmholtz puts forward in support of the view that beats cannot form any note whatever (*Tonempfindung*, pp. 245, 263). But if, on the one hand, summation-notes do not coincide with the beats, yet on the other hand, as we have seen above, neither the beat-notes of the intervals $n : n + m$, if m is much greater than $\frac{n}{2}$, nor the beat-notes of all intervals $n : hn + m$ coincide with the difference or the sum of the primary notes; and the beat-notes are therefore as little demonstrated by the cause which produces the combination-notes as the latter can be demonstrated from the existence of the beats; and we must consequently suppose that each of these species of notes has its peculiar origin.

As to the question whether the nature of the beats will itself admit of their forming themselves together into a note, the circumstance that, when the vibrations of the primary notes are not infinitesimally small, combination-notes of the difference and the sum ensue, can of course prove nothing either for or against this view. Helmholtz, however, gives some other reasons against the older opinion of Th. Young, which require a closer inquiry in order to refute them.

The way in which the beats in the ordinary (and particularly therefore in the lower) parts of the scale mostly produce very weak notes is that which has principally induced Helmholtz to declare that vibrations of simpler notes, without any mixture of upper notes or combination-notes, "only arise when the two given notes are divided from one another by a tolerably small interval," and that, "when the interval is increased by only the amount of a minor third, their vibrations become indistinct" (*Tonempfindung*, p. 284). But if we use deep and sufficiently powerful notes, the primary beats, as I have mentioned above, are audible in considerably greater intervals. In the octave C...c there is no interval which does not allow them to be clearly heard; and even if we set aside the beats m' , we can follow the beats m alone to above the fifth; and in intervals with the fundamental note double E they may be noticed even close to the seventh.

In the above Table I have stated that the third $c : e$ allows a rattle of 32 beats to be heard, and that this ever lessening rattle may be followed as far as the fifth. These results only refer to the clang of primary notes of such strength as my tuning-forks, placed in front of resonators, produced. When, however, I made use of the louder notes c , e , and g which I produced by sounding the forks in question without

sliding weights in front of large suitable sounding-boxes open at both ends, the rattle of the third was still more powerful, and that of the fifth also much louder. The 64 beats of the third $c':e'$, which with the tuning-forks and resonators only allow a mere roughness to be heard, changed by means of the tuning-forks on sounding-boxes to a positive rattle; and even the fifth $c':g'$ allowed a trace of the roughness produced by 128 beats to be heard.

When a note is produced in a closed space, it is well known that, by the combination of the direct sound-waves and those that reverberate from the walls, nodes and ventral segments are formed. In very loud simple notes, of which the sound-waves are tolerably long, the difference of intensity at these different points is so remarkable that in the above-mentioned experiments, in which it is above all necessary that the ear should receive both notes very strongly, we must be careful to place it for both notes at the same node. The ear must therefore be placed in the best position for one note, and then the other fork moved away till its note also is heard with the greatest intensity. The higher we go in the scale, the easier it is to produce very powerful piercing notes; and while the interval of the fifth $c':g'$, which with ordinarily powerful notes allows no trace of roughness to be perceived, must be produced by notes more powerful than can be found in any musical instrument in order that its 128 beats may be perceived, for the notes $b''':c^{iv}$ the reeds of a harmonium suffice to make the same number of beats audible.

Helmholtz, who states this last fact, lays, in order to explain it, particular weight upon the smallness of the interval (*Tonempfind.* p. 263); but, as will be seen from the above-mentioned experiments with deep and very powerful notes, it is only necessary to make use of primary notes of sufficient intensity in order to obtain the same phenomenon with much greater intervals, while on the other hand, again, with sufficiently faint high notes very small intervals may be formed which do not allow it to be perceived.

As the small intervals of higher notes, with regard to the audibility of the single beats, cannot be distinguished from wider intervals of sufficiently powerful deeper notes which are separated from one another by an equal absolute number of vibrations, so also they show no difference in the manner in which beat-notes are formed. Two tuning-forks, b''', c^{iv} (15:16), with a tolerably intense rattle of 128 beats, allow the note c to be heard, just as with the very powerful notes c' and g' ; besides the roughness a faint c is perceived; but it must be observed that, as these high primary notes possess a

proportionally far greater intensity than the deeper ones, their beat-notes are also much more powerful than the beat-notes of the same pitch which are produced by wider intervals of deeper notes, and that it is therefore much easier to produce very deep clearly audible beat-notes from them than from deeper primary notes.

I have stated above that the clang of $c : g$, even when very powerful tuning-forks and resonators are used, only allows a scarcely audible C (128 v. s.) to be perceived, and deeper beat-notes in the lower parts of the scale I could not directly observe at all; but with high forks it is possible to produce even the double C of 32 complete vibrations, which lies on the furthest limits of audibility.

The first series of tuning-forks which I made use of for this experiment were tuned to notes between b''' and c^{iv} ; as, however, these forks only allowed the beat-notes of 40 and 36 complete vibrations (double E and double D) to be heard as faintly as possible, I constructed a second series for notes between b^{iv} and e^v , which gave a proportionally far greater intensity. Such powerful beat-notes are produced by the latter, that not only are, for example, c and C distinctly to be heard at a considerable distance, but also all the notes of the deep octave to double C can be clearly distinguished. This latter is produced by the notes 4064 and 4096 v. s., which stand in the ratio of 127 : 128, and form thus an interval far smaller than a comma (80 : 81).

The following Table contains all the tuning-forks which form the two just mentioned series, with their ratios, and the beat-notes formed from them.

v. s.	v. s.		Beats.	Note.
3840	: 4096	15 : 16	128 = C.	
3904	: „	61 : 64	96 = G.	
3936	: „	123 : 128	80 = E.	
3968	: „	31 : 32	64 = C.	
3976	: „	497 : 512	60 = Double B.	
3989·3:	„	187 : 192	53·3 = „	A.
4000	: „	125 : 128	48 = „	G.
4010·7:	„	47 : 48	42·7 = „	F.
4016	: „	251 : 256	40 = „	E.
4024	: „	503 : 512	36 = „	D.
7936	: 8192	31 : 32	128 = C.	
8064	: „	63 : 64	64 = C.	
8096	: „	253 : 256	48 = Double G.	
8106·7:	„	95 : 96	42·7 = „	F.
8112	: „	507 : 512	40 = „	E.
8120	: „	1015 : 1024	36 = „	D.
8128	: „	127 : 128	32 = „	C.

In making these experiments we can, as usual, strike the forks with a bow ; but as in consequence of their high pitch there is no longer any fear of the formation of *partial notes*, it is often more convenient to strike them with a steel clapper, as this moves more quickly, and the note of the fork first touched has not then lost much of its intensity when the second note is produced.

All the clangs given in this Table allow the rattle (or, as it is better termed in these high notes, the whir) of the beats to be heard simultaneously with the beat-notes, which latter are more powerful according as the tuning-forks are struck harder. If we wish to hear the whir of the beats alone, we have only to remove the two forks a little further from the ear ; the beat-notes, however, cannot be distinguished quite alone, even if we place the forks close to the ear ; we cannot even quite succeed in doing this with the notes 7936 and 8192 v. s., although with these the beat-note *c* is extremely powerful.

We see by these experiments that with sufficiently powerful primary notes, not more than 32 beats are necessary to form a note, that further beats to about 128 can be distinguished in intervals of any extent that may be wished, and that between 32 and about 128 beats in the second the beats and beat-notes can be heard together. The question now is whether this is the same result which we obtain also with primary impulses.

It is known, in the first place, that 32 primary impulses can form a note; and we might expect, on the other hand, that the ear should be capable of perceiving more than 100 impulses in the second, even from the old observations, according to which it can perceive the difference of movement between two pendulums which do not diverge from isochronism by more than the hundredth part of a second. It was to be supposed, indeed, that if the ear could receive two distinct impressions only $\frac{1}{100}$ of a second apart, it could also perceive a whole series of such effects with similar distances ; but this experiment can also be very well directly made with a cog-wheel. That which I used is of wood, 35 millims. thick, 36 centims. in diameter, and with 128 teeth. If we press a small spring board of hard wood very strongly upon these teeth, we hear through the constantly increasing rapidity of rotation the first hardly perceptible beats change to a rattle, which is still clearly perceived when the wheel revolves once in the second, and consequently the number of strokes has attained to 128. Besides this rattle we can also hear, however, if the single strokes are not too powerful, the note *c* (256 v. s.). If we re-

place the little wooden board which strikes so hard by a piece of card, the rattle is hardly to be discerned at all, and the note *c* stands out with greater clearness. If we turn the wheel only once in two seconds, so that we only produce 64 strokes in the second, we can still more easily observe the almost entire disappearance or withdrawal of the note *C* from the rattle of the 64 strokes. There exists consequently the most perfect agreement between the ratio of primary impulses and that of the beats.

It is obvious that the simultaneous audibleness of single strokes and of the notes which arise from their sequence, as also the cessation of the audibleness of single strokes when these surpass a certain number, are fully explained by the theory of hearing put forward by Helmholtz. According to this theory, as is known, there exist in the ear certain elastic bodies "greatly muffled" (*Tonempfindung*, p. 226), which serve for the perception of swiftly passing irregular shocks—and also "less-muffled elastic bodies," which are much more powerfully affected by a musical note of correspondingly high pitch than by single beats. Each of the single beats produces, therefore, an impression upon a body of the former sort, so long as these strokes do not succeed each other in a shorter interval of time than is necessary for the muffling of the concussion produced in it. But, further, the periodical movement produced by the sequence of the strokes is composed of a sum of vibrations like those of the pendulum—that is to say, of simple notes, which can each affect an elastic body of the second nature. The more, therefore, the movement of the air caused by single strokes differs from the simple pendulum movement, the greater will be the perceptibility of the single strokes and the weaker the intensity of the note arising from their sequence; while, on the other hand, the intensity of the latter increases, and the audibleness of the single impulses becomes weaker, as this periodical movement approaches nearer to the simple pendulum movement; so that at last, with almost entirely simple pendulum vibrations as they are produced by tuning-forks, at above 32 and 36 nothing more is perceptible of the single impulses, and the note only is heard.

Helmholtz has remarked further that an undulating clang may be compared to a note of periodically changing intensity, and that "undulations and intermissions resemble each other, and also that at a certain number they produce that kind of noise which we call a rattle" (*Tonempfind.* p. 266). If intermissions, then, always produced only a rattle, the great resemblance which they show when not too numerous to the vibrations might make us suppose that these latter are only capable of producing a rattle; but intermissions, just like

primary impulses, at a sufficient number and intensity change into a note.

This may be easily demonstrated by means of a disk in which is a circle of large holes, and which is made to revolve before a tuning-fork. I have used different disks with 16, 24, and 32 holes, 20 millims. in diameter, at various distances, each disk much larger than the circle of holes, so that the note, as far as possible, should only penetrate strongly to the ear when an opening was in front of the tuning-fork. Of course any particular note will not, in any particular number of intermissions, produce a note which corresponds to this number of intermissions; but it will be necessary, besides the needful strength and the sufficient number of intermissions, that the air-shocks which penetrate through the openings of the disk shall be equal to each other; and this cannot be, for instance, when the number of intermissions is greater than the number of double vibrations of the note. In this case either several holes pass by the same sound-wave, so that a fresh part of this wave always passes through each, or, at any rate, they are not equal parts of different sound-waves which the openings make a way for to the ear. When, too, the number of the intermissions is only a little greater than the number of double vibrations of the note, similar conditions ensue, and it becomes necessary that at least one entire sound-wave should penetrate through the opening in order that the intermission-note may be clearly perceptible. The most favourable circumstance for its audibleness seems to be that in which an entire series of sound-waves can penetrate through each opening—that is, when the vibrations of the note are considerably more in number than the intermissions.

If a disk in which the distance of the holes from one another is three times as great as their diameter (2 centims.) is allowed to move with such rapidity that 128 holes pass the tuning-fork in a second, the intermitting note c is heard with the fork $c'' = 512$ v. d.; but it is faint, and is less prominent than the two variation-notes, which equal the difference and the sum of the intermitting notes and of the double vibrations of the fork, and which are therefore here $g' = 384$ v. d. and $e'' = 640$ v. d. (*Tonempfind.* p. 628). If, while the disk moves always with the same rapidity, the forks e'' , g'' , seventh harmonic of c , and c''' are used one after another, the intermitting note increases constantly in strength and clearness. If, lastly, the notes of the very powerful forks c^{iv} and c^v are allowed to penetrate through the holes of the disk when the ratio between the number of intermissions and that of the double vibrations of the note is 1 : 16 and 1 : 32, the intermitting note is extra-

ordinarily powerful; while the difference- and summation-notes 15 and 17 are but faintly perceptible at 1:16, and the notes 31 and 33 at 1:32 can scarcely be perceived at all.

In experiments with the last-named tuning-forks, which are therefore the most favourable for the observation of the intermitting notes, I move the disk directly in front of the forks. When, however, I use deeper forks, I insert between them and the disk suitable resonators of the same diameter as the holes in the disk, so that the note always sounds loudly when one of these holes is in front of the opening in the resonator. It may be remarked, by the way, that with this arrangement the variation-notes especially sound wonderfully beautiful, and when the disk is moved alternately quicker and slower they may be distinctly heard to retreat from and approach one another.

In the above, only a note of constantly equal intensity was allowed to approach the ear intermittingly by mechanical means; the transition of periodical vibration maxima to a note, however, can also be observed in notes which themselves possess a periodically changing intensity. For this purpose I have constructed siren-disks with circles in which the holes are at equal distances, but get periodically larger and smaller, so that a series of isochronous impulses of periodically changing intensity is produced if they are sounded through reeds of the same diameter as the largest holes. One of these disks contained three circles, each of 96 equidistant holes, whose diameter varied on the first circle 16 times from 1 to 6 millims., on the second 12 times, and on the third 8 times. If these circles were sounded with a tube of 6 millims. diameter, while the disk was at first turned slowly, the single-hole periods were heard in each circle like separate beats; when it was turned faster, first the 16 periods of the first, then the 12 of the second, and lastly the 8 of the third circle changed to one note; when, lastly, the high note of 96 holes with 8 turns of the disk had reached the second g'' , the deep notes C, G, and double C, corresponding to the number of periods, were clearly and powerfully heard with this g'' .

On another still larger disk, 70 centims. in diameter, I arranged seven circles of 192 equidistant holes, which periodically increased and decreased in size 96, 64, 48, 32, 24, 16, and 12 times. In the first, therefore, a whole period was contained in two different large openings, and the note of the periods in it was therefore only an octave deeper than the note of the 192 holes, while in the seventh circle each period was formed of 16 openings, and the note of the periods was consequently four octaves deeper than the note of the 192

holes. Notwithstanding this great difference in the number of the primary impulses which the single periods produced upon these different circles, they all equally, when their number had become sufficiently great, changed into a note; and if the circles, following the series from the seventh to the first, were blown upon, they always allowed the deep note in the interchanging series of the fourth and fifth to be clearly and loudly heard beside the constantly unchanging high note.

Although, therefore, such series of isolated impulses of periodically changing intensity show a great likeness to clangs producing beats (with regard to the possibility of the single maxima of intensity changing to a note), they are still very different from the latter. If, for example, a series of 96 isochronous impulses increasing and decreasing sixteen times in intensity imitates very closely the clang of two notes which allow 16 beats to be heard, the two primary notes should be perceptible which form this simultaneous sound—in this case 88 and 104, two notes in the interval 11 : 13; but in fact we cannot hear them. The reason of this may certainly be looked for in the fact that two notes near unison, whose number of vibrations are a and b , periodically exhibit when sounded together an

increase and decrease of vibrations of about $\frac{a+b}{2}$, but that at the change from one period to another a change of signs takes place, so that the maxima of compression of the middle vibrations are only isochronous in the imperfect periods, while at the perfect periods the maxima of dilatation take their place.

I have endeavoured in two different ways, by means of primary impulses, approximately to obtain this result, and first by producing the resultant compressions of all the following vibrations of the clang on the same circle of the disk of a siren through holes of appropriate size. The clang of two notes of 80 and 96 double vibrations produces a note of

$$\frac{80+96}{2} = 88 \text{ vibrations, increasing and decreasing in intensity}$$

16 times; and at each change from one beat to another the change of signs causes the maximum of compression of the first vibration of the following undulation to vary from the maximum of compression of the last vibration of the preceding undulation by half a vibration. I therefore divided the circle into 176 parts, and in parts 1, 3, 5, 7, and 9 bored five holes of different sizes, the same thing in parts 12, 14, 16, 18, and 20, again in parts 23, 25, 27, 29, and 31, and so on. If now a circle of holes was blown upon through a tube of the diameter of the

largest opening we could certainly perceive, beside the note 88 and the very powerful note of the period 16, the two notes 80 and 96; but they were very faint, and on account of the great roughness of the deep note somewhat difficult to observe.

In the second arrangement I endeavoured directly to imitate the change of phases in the vibrations in the change from one undulation to another. For this purpose I divided two concentric circles working close together into 88 parts, and disposed the openings which were to produce successive undulations alternately upon the two. As with 88 openings and 16 periods $5\frac{1}{2}$ holes would have come upon each of the latter, I always took two periods together, and bored therefore on the first circle the divisions 1, 2, 3, 4, 5, 6, and on the second 6, 7, 8, 9, 10, 11; then, again, on the first circle the divisions 12, 13, 14, 15, 16, 17, and on the second 17, 18, 19, 20, 21, 22, and so on. If these two circles of holes were now blown upon, one from above and the other from below, through two tubes each of the diameter of the largest opening of its own circle, there ensued at each revolution of the disk a series of 88 isochronous impulses, varying periodically 16 times in intensity, which at every change from one intensive period to another changed their signs. In this experiment the two notes 88 and 96 appeared much more distinctly than in that previously described with the circle of holes blown upon from one side, which had upon it the periods of holes divided from one another by the length of a half vibration.

It only remains further for me to mention that Tyndall has cited the slight intensity of the resultant notes as a proof that they cannot have been produced by the beats of the primary notes (*On Sound*, p. 350). After setting forth clearly that, when two equally powerful notes produce beats, the note always changes periodically from cessation to a doubly greater amplitude than either of the primary notes singly had had, Tyndall says literally:—"If, therefore, the resultant notes were due to the beats of their primaries, they ought to be heard even when the primaries are feeble; but they are not heard under these circumstances." Now, of course, beat-notes must always have a greater intensity than their primary notes, if vibrations of equal amplitude always produced the same intensity in all notes; but this is not the case, as may be proved by a very simple experiment. If a *c* tuning-fork, vibrating in the amplitude of 1 millim., is held so far from the ear that the note is barely audible, and if at the same time the same experiment is made with a second, *c'* fork, whose prongs are of the same thickness and breadth, while it also vibrates in an amplitude of 1 millim., it will be found that

it must be held about double as far from the ear in order to produce the same result ; and it follows therefore that the note c' , with the same distance of vibration, is about four times as powerful as the note c . If we try, then, to make both forks vibrate in such an amplitude that at the same distance from the ear about the same result is produced, it will be found that the amplitude of the c fork must be about four times as great as that of the c' fork. According to this the amplitude of two equally powerful notes in the interval of the fifth, *e.g.*, must be 9 and 4, and the sum of these amplitudes would then be 13 ; but the resultant note, which is an octave deeper than the fundamental note of the fifth interval, would require an amplitude of vibration of 36, in order to acquire the same intensity as the primary notes singly possess.

If the interval of the primary notes is still smaller, the beat-note falls still lower, and must therefore be weaker in proportion to the intensity of the primary notes. It stands to reason that I do not give the above-mentioned experiments, nor the numbers in the example as quite exact ; but they are sufficiently so to show what convinced me that deep notes must have a far greater amplitude of vibration than high ones, in order to equal the latter in intensity. I hope to be able to return before long to a closer examination of the intensity of different high notes.

The most important results of the above-mentioned experiments are shortly summed up as follows :—

(1) The number of beats of two notes n, n' is always equal to the positive and negative remainder of the division $\frac{n'}{n}$; that is, equal to the numbers m, m' , which are produced by stating $n' = h n + m = (h + 1) n - m'$, where n, n' is the number of the double vibrations, and h the quotient of the division which gives the remainder m . It is as if the beats proceeded from the two overtones h and $h + 1$ of the lower note n , between which the higher note n' lies. The cause of the beat-notes is simply the periodical coincidence of the common maxima of the two sound-waves.

(2) The beats of the pure harmonic intervals can be heard in the relations 1 : 8 and even 1 : 10, and may, as well as the beats of the unison, be regarded as resulting directly from the composition of the vibrations of the primary notes, without the help of resultant intermediate notes, whose existence cannot be proved.

(3) Both the beats m and the beats m' , not only of the

interval $n : n + m$, but also of the interval $n : h n + m$ ($h = 2, 3, 4$), when the intensity of the primary notes and their number are sufficient, change into beat-notes.

II. (4) When the two beat-notes m and m' are near the unison, the octave, and twelfth, the same beats may be heard as would be produced by two equal primary notes. I have named these beats arising from beat-notes secondary beats, in order to distinguish them from the beats arising from primary notes.

(5) When the intensity of the beat-notes by which they are formed and their number are sufficient, these secondary beats change to a secondary beat-note, as primary beats change to a primary beat-note.

III. (6) The difference-notes and summation-notes, which are produced by the clang of two loud notes (the vibrations of the latter not being infinitesimal), produce a phenomenon which is independent of the beats and beat-notes: they are very much weaker than the beat-notes.

IV. (7) The beat-notes cannot be explained by reason of the difference-notes and summation-notes, because the number of their vibrations is in many cases different from what this cause might produce.

(8) The audibility of the beats depends solely upon their number and upon the intensity of the primary notes, and is independent of the distance of the interval.

(9) The number of the beats and of the primary impulses in which both may be perceived as separate impulses is the same.

(10) With the beats perceived as separate impulses, as with the primary impulses perceived in the same manner, the note which approaches them in number is audible.

(11) The number at which beats and primary impulses can change into one note is the same.

(12) As with beats and primary impulses, the intermissions of a note can also change into one note.

(13) When the vibrations of a note vary periodically in intensity, the periodical maxima of vibration change into one note, if their number is sufficient.

(14) The beat-note which is formed by two primary notes must always be weaker than the latter, although single beats are stronger than the notes which form them.

Paris, December 1875.

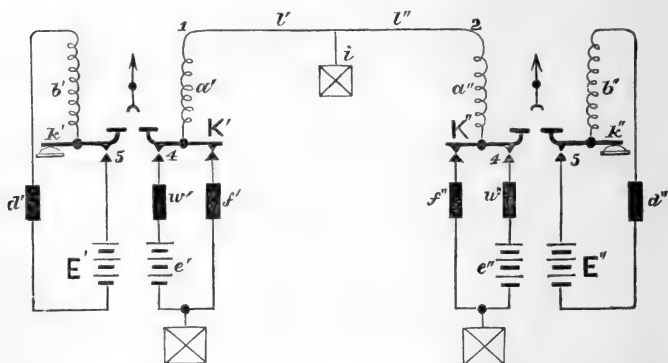
LXII. *On the General Theory of Duplex Telegraphy.*
By LOUIS SCHWENDLER *.

[Continued from S. 4. vol. 1. p. 475.]

III. *The Compensation Method* †.

THIS method is the oldest; fig. 3 gives the general diagram.

Fig. 3.



e is the electromotive force of the line battery.

β its internal resistance.

E is the electromotive force of the compensation battery.

α its internal resistance.

K is a constant-resistance key. Dr. Gintl used an ordinary key, which, it will be obvious, must result in a failure.

k is an ordinary key: both keys, in the same station, are worked simultaneously; *i. e.* contacts 4 and 5 are closed and broken at one and the same time.

$d, f,$ and w are certain resistances.

a is the *one* coil of the differential instrument, which is connected up in the line-circuit.

b is the *other* coil of the differential instrument, which is connected up in the compensation circuit. By a and b shall be also designated the resistances of these two coils.

The coils a and b with their batteries e and E respectively are arranged in such a manner that they have opposite magnetic effects with respect to the same magnetic pole. The two circuits in each station (the line-circuit and the compensation circuit) are insulated from each other. All the other terms, as $L, L', L'',$ &c., shall have the same physical meaning as before.

* Communicated by the Author from the Journal of the Asiatic Society of Bengal, vol. xlv. part 2, 1875.

† Dr. Wilhelm Gintl, k.-k. Director-General of Telegraphs in Austria, is the inventor of this earliest method. In 1853 he made the first practical experiment on a line between Vienna and Prague (240 miles).

The compensation method has two principal defects which the two preceding methods do not possess.

First. The success of working a line *dupliciter* by the compensation method will clearly depend on the possibility of being able to close and open simultaneously two different contacts (4 and 5). The mechanical difficulty of doing so sufficiently accurately was pointed out by Dr. Werner Siemens, and in fact constitutes one of the reasons which led him to propose the differential method.

Secondly. The balance in each station may be disturbed *directly* by a variation of the electrical condition (internal resistance and electromotive force) of the two batteries (E and e) employed.

In the preceding two methods the variation of the internal resistance of the signalling-battery can only be felt *indirectly* by affecting the balance of the distant station, while the variation of electromotive force has no effect at all. Hence a given variation in the battery or batteries must necessarily produce a greater disturbance of balance in the *compensation method* than in the two preceding ones. We know that even so-called constant galvanic batteries, doing work, alter their electrical conditions perceptibly, especially their internal resistance; and consequently this defect weighs most decidedly against the compensation method. In all other respects the compensation method has the same defects as the differential method, and in addition some others which will be understood as the investigation proceeds.

General expression for the two functions "D" and "S."

To obtain the functions D and S, we have to develop the general expressions for the forces p , P , and Q , say (for station I.)

$$p' = A'm' - B'n',$$

where A' and B' are the currents which pass through the two coils a' and b' respectively when station I. is sending and station II. is at rest; m' and n' are the forces exerted by the two coils a' and b' respectively on one and the same magnetic pole when a unit of current passes through them. At balance in station I.

$$p' = 0.$$

Further,

$$P' = \mathfrak{A}'m',$$

where \mathfrak{A}' is the current which passes through the coil a' when station II. is sending and station I. is at rest (single signals).

Further,

$$Q' = \mathfrak{A}'m' + \mathfrak{B}'n',$$

where ∇' and \mathfrak{A}' are the currents which pass through the coils a' and b' respectively when both stations are sending simultaneously (duplex signals).

The compensation circuit and the line-circuit in each station being electrically independent of each other, we have

$$\mathfrak{A}' = B'$$

invariably without condition.

If we further presuppose that depressing of the key K does not alter the complex resistance of the station (a condition which, for the regularity of signals, we are obliged to assume here as well as in the two preceding methods), it will be clear that

$$\nabla' = A' + '.$$

Substituting these values for ∇' and \mathfrak{A}' in the expression for Q' , we get

$$p' = A'm' - B'n',$$

$$P' = \mathfrak{A}'m',$$

$$Q' = (A' + \mathfrak{A}')m' + B'n'.$$

The signs of the terms may be again contained in the currents, while m' and n' are taken as absolute numbers. We must only remember that $A'm'$ and $B'n'$ must be invariably of opposite sign. Arbitrarily we will call the current A positive when the negative pole of the line-battery is to earth.

Now we have again two different modes of connecting up the line-batteries, viz. :—

1st. The same poles of the line-batteries are connected to earth in the two stations :

$$p' = \pm A'm' \mp B'n',$$

$$P' = \mp \mathfrak{A}'m',$$

$$Q' = (\pm A' \mp \mathfrak{A}')m' \mp B'n'.$$

2nd. Opposite poles of the two line batteries are connected to earth in the two stations :

$$p' = \pm A'm' \mp B'n',$$

$$P' = \pm \mathfrak{A}'m',$$

$$Q' = (\pm A' \pm \mathfrak{A}')m' \mp B'n'.$$

Subtracting in either case P' from Q' , we get

$$Q' - P' = S' = p'.$$

Or, on account of having fulfilled the key-equation $f = w + \beta$, the difference of the forces which produce single and duplex signals is equal in sign and magnitude to the force by which balance is disturbed. Further, it is also, for the compensation

method, quite immaterial whether the same or opposite poles of the two line-batteries are connected to earth. As pointed out, it is preferable to connect the same poles, *i. e.* the negative poles, of the line-batteries to earth.

Assuming this case, we have

$$\begin{aligned} p' &= A'm' - B'n', \\ P' &= -\mathfrak{A}'m', \\ Q' &= (A' - \mathfrak{A}')m' - B'n'. \end{aligned}$$

Substituting now for A' , B' , and \mathfrak{A}' their values, and remembering that

$$\left. \begin{aligned} m' &= q' \sqrt{a'}, \\ n' &= r' \sqrt{b'} \end{aligned} \right\} \text{approximately,}$$

we get the following general expressions for the two functions D and S :—

$$\left. \begin{aligned} S' &= e' q' \frac{\Delta'}{R'K'}, \\ D' &= \frac{e'}{e''} \cdot \frac{K''}{R'K'} \cdot \frac{\Delta'}{\mu' \sqrt{a'}} \end{aligned} \right\} \text{for station I.}$$

and

$$\left. \begin{aligned} S'' &= e'' q'' \frac{\Delta''}{R''K''}, \\ D'' &= \frac{e''}{e'} \cdot \frac{K'}{R''K''} \cdot \frac{\Delta''}{\mu'' \sqrt{a''}} \end{aligned} \right\} \text{for station II.,}$$

where

$$\Delta = R \sqrt{a} - K\lambda v \sqrt{b},$$

$$R = \alpha + b + d,$$

$$K = f + a + c,$$

$$\lambda = \frac{E}{e},$$

$$v = \frac{r}{q}.$$

Rigid fulfilment of the two functions $S=0$ and $D=0$.

For finite quantities these two functions can only become zero if $\Delta=0$, *i. e.*

$$R \sqrt{a} - K\lambda v \sqrt{b} = 0,$$

which is the balance-equation for the *compensation method*.

To fulfil this equation permanently, no matter what the special cause of disturbance may be, we can again adopt two essentially different modes of readjustment, *viz.*:—

Either leave the two coils a and b or their armatures stationary.

tionary, and adjust balance by altering the resistance in one or both of the two circuits, or leave the resistances constant and alter the relative position of the two coils or their armatures with respect to a given magnetic pole. These two methods of readjusting balance shall be considered separately.

a. *Readjustment of balance by altering resistances.*

In order to have *immediate balance* it will be clear that the alteration of resistance must be restricted to the compensation circuit, which is electrically independent of the line-circuit. The total resistance in the compensation circuit consists of three different resistances, namely b , a , and d . Neither b nor a , considering their nature, can conveniently be made adjustable in practice; hence the alteration of resistance in the compensation circuit is restricted to d , which must therefore consist of increments of the proper size. The adjustment of d should be quick and convenient.

In addition to this adjustment, $\lambda = \frac{E}{e}$ may be made adjustable by varying E in increments of *one cell*. Such an adjustment, however, is not fine enough for ordinary use; the electromotive force of one cell is too large a quantity in comparison with the total electromotive force used in the compensation circuit. If the variation of the line-current becomes very great, it might perhaps be found convenient to alter E ; but as an ordinary mode of adjustment it must be dispensed with*.

It is scarcely needed to point out that to adjust balance by altering the line-current, either by varying the resistance or the electromotive force† or both of the line-circuit, must be rejected once for all, because such an adjustment of balance in the one station could never take place without disturbing the balance of the other station, or, in other words, the required *immediate balance* could not be fulfilled.

b. *Readjustment of balance by moving the coils or armatures.*

If we suppose both the coils or their armatures simultaneously movable in the same direction, then clearly this mode of adjustment contains not only the required *immediate balance*, but in addition represents also a very rapid and entirely continuous action. For this reason it is apparently preferable to

* During the period of low insulation of the line it might be advisable and practicable to make E larger than during the period of high insulation of the line (wet and dry seasons).

† Alteration of electromotive force of a galvanic battery cannot be achieved without altering its internal resistance. Hence varying e would also involve a variation of β ; and in order to keep $f = w + \beta$, it would become necessary to alter w simultaneously with e ; i. e. w would have to be increased when e decreases, and *vice versa*. This method, besides being rough, would therefore be also inconvenient.

the first method, where the adjustment can only be carried on in *one* branch by varying d in increments *. Which of the two methods, however, is to be finally chosen, depends on other considerations, which will become clear further on. We know now that both these modes of adjustment are convenient and practicable, and contain *immediate* balance without special conditions. In fact in this respect the compensation method is preferable to the differential method, where immediate balance by varying resistances could only be obtained when varying the four branches simultaneously according to a fixed relation.

Rapid approximation of the two functions S and D towards zero.

On account of $f = w + \beta$ we have

$$S = p = eq \frac{\Delta}{RK},$$

where

$$\Delta = R\sqrt{a} - K\lambda v\sqrt{b}.$$

Now suppose $\Delta = 0$; then this equation may be disturbed by K , R , λ , v , a , or b varying. a and b are wire resistances which

* It has been suggested to adjust balance by a continuous variation of resistance, as, for instance, by moving a contact-point along a thin platinum wire in the same manner as Dr. W. Siemens has done it in his bridge employed for comparing accurately comparatively small resistances. It is, however, scarcely necessary to point out that such a method, if applied for duplex working, must result in a failure, at all events so long as electromagnetic instruments are used for producing the signals. For in such a case the resistance of any branch, no matter what special duplex method may be employed, must bear a certain ratio to the given resistance of the line in order to get the signals with sufficient force. This ratio, as my investigations have shown, is by no means a small one; and hence the resistances of all branches, even for a short line, cannot be made small. Therefore the platinum wire constituting part of one or two branches of the duplex method employed, must also offer a considerable resistance, *i. e.* must be of great length. Hence to alter such a large resistance continuously and perceptibly, as is indicated by the balance-disturbance, must evidently involve a considerable movement of the contact-point, which, even choosing the thinnest possible wire and the shortest telegraph-line, becomes already for the daily variation so large as to make its application impossible. Unless another material of much higher specific resistance than platinum wire can be found, which at the same time allows of the sliding contact being made securely, the adjustment of balance by a continuous variation of resistances must be dispensed with. Such a material does not appear to exist. I thought of acting on Phillips's suggestion to use pencil-marks for the adjustable resistance; and although I found that pencil resistances can be adjusted very accurately, and can be enclosed in a very small space, and that they keep sufficiently constant, it is difficult, if not impossible, to alter them by a sliding contact; the "*Uebergangswiderstand*" is too variable and too great. Besides, if the contact is made with sufficient pressure, its sliding along alters the thickness of the pencil-mark, and hence the resistances become inconstant and uncertain.

may be taken as constant, for their variation with temperature is exceedingly small ; and in case of accident (*i. e.* a coil breaking or becoming shunted) nothing short of actual repair could help. Further, v , supposing the differential instrument to be properly designed and mechanically well executed, may be taken as a perfectly constant quantity, which certainly, as long as the coils or their armatures are not moved on purpose, does not alter of its own accord.

The quantities left, which by variation may affect the balance-equation, are K , R , and λ .

Of these three quantities the variation of K may become largest ; for K does not only contain the line-resistance, which is highly variable, but K includes also the internal resistance of both the line-batteries, which, even for the best known form of galvanic battery, is by no means a constant quantity. The variation of the internal resistance of the line-battery in each station produces, of course, the greatest disturbance of balance in *that* station.

The next quantity most liable to change of its own accord is clearly R , since it contains the internal resistance of the compensation battery.

λ , the ratio of the two electromotive forces in one and the same station, though also liable to change, will yet vary very little. The electromotive force of a well-prepared galvanic battery, especially when the battery is worked by weak currents, is far more constant than is generally believed*.

With respect to the variation of the three quantities K , R , and λ , the function S may therefore be expressed in three different forms.

$$S_1 = eq \frac{\lambda v \sqrt{b}}{RK} \delta K \text{ when } K \text{ alone varies.}$$

$$S_2 = eq \frac{\sqrt{a}}{RK} \delta R \text{ when } R, \text{ i. e. } \alpha, \text{ alone varies.}$$

$$S_3 = eq \frac{v \sqrt{b}}{R} \delta \lambda \text{ when } \lambda, \text{ i. e. } E \text{ or } e \text{ or both, alone vary.}$$

These three different disturbances of balance may act singly or conjointly ; and it is clear that they are independent of each

* It appears that changes which have been observed to take place in the electromotive force of a Minotto or Leclanche's battery are generally apparent only, not real. Such changes are generally quite within the limits of observation-errors ; and if they are large, they are then generally due to the incorrectness of the method employed for measuring the electromotive force, or to cells actually having become exhausted. It appears that this mysterious force in each cell either exists in its full vigour or not at all : there seems to be no continuous change in either direction.

other, at all events as far as this investigation is concerned. Consequently the safest plan will be to make each influence as small as the circumstances will allow it.

The disturbance S_1 for any constant $eq\lambda v\sqrt{b}$ and any given δK will obviously become smaller the larger RK is selected. Supposing $R + K$ constant, whatever that value finally may be, RK has a maximum for $R = K$; and the very same condition will obviously make the disturbance S_2 smallest.

S_3 offers no best condition; this expression only shows that it has an absolute maximum with respect to b , namely (as $R = a + d + b$) for $b = a + d$.

Thus we are informed that whatever relation between b and $a + d$ may be finally chosen, $b = a + d$ should *not* be selected, as otherwise any given variation of λ would have the greatest possible disturbing effect on the balance. But $b = a + d$ being the condition for the maximum magnetic effect in the compensation circuit, it is hereby established that for the sake of regularity of signals, which under all circumstances is to be considered of paramount importance in duplex telegraphy, the magnetic effect in the compensation branch *must not* be achieved in the most economical manner, but quite the reverse. This, as the compensation circuit has actually to produce wholly or partly the *duplex signals*, is a *testimonium paupertatis* for the compensation method, and proves it in this respect inferior to both the *double balance* and the *differential method*.

$$R = K$$

is the regularity condition for the compensation method; *i. e.*

In order to make the disturbance of balance by a variation of the resistance in both the circuits absolutely as small as possible, the total resistance of the compensation circuit should be equal to the total resistance of the line-circuit.*

If we now substitute in S_1 for K the value R , and in S_2 for R the value K , we get

$$S_1 = eq \frac{\lambda v \sqrt{b}}{R^2} \delta K,$$

$$S_2 = eq \frac{\sqrt{a}}{K^2} \delta R,$$

* This result is against the adopted view, for Dr. Gintl, as well as others after him, has always treated the compensation circuit as a kind of *local circuit*, *i. e.* giving to it as low a resistance as practice allows. But this is clearly wrong; for if R is made very small as compared with K , the balance becomes unstable. This fact explains, to a certain degree, the failure which has attended the application of the compensation method for duplex working, because the method was tried under the most unfavourable quantitative arrangements.

while

$$S_3 = eq \frac{v \sqrt{b}}{R} \delta \lambda$$

remains the same.

S_1 has an absolute maximum for $b = \frac{\alpha + d}{3}$, S_2 for $a = \frac{f + c}{3}$, and S_3 for $b = \alpha + d$, as stated before.

Hence we know what relations between the different variable should *not* exist.

This is all we can get from the function S. For further relations we must look to the function D.

For station I. we have *

$$D' = \frac{e'}{e''} \frac{K''}{R'K'} \cdot \frac{\Delta'}{\mu' \sqrt{a'}},$$

which, again, with respect to the variations of K' , R' , and λ' , may be written in three different forms:—

$$D'_1 = \frac{e'}{e''} \frac{K''}{R'K'} \cdot \frac{\lambda' v' \sqrt{b'}}{\mu' \sqrt{a'}} \delta K',$$

$$D'_2 = \frac{e'}{e''} \cdot \frac{K''}{R'K'} \cdot \frac{1}{\mu'} \delta R',$$

and

$$D'_3 = \frac{e'}{e''} \cdot \frac{K''}{R'} \cdot \frac{v' \sqrt{b'}}{\mu' \sqrt{a'}} \delta \lambda'.$$

Considering that

$$\frac{K''}{K'} = \frac{i + l'' + \rho''}{i + l' + \rho'},$$

$$\mu' = \frac{i}{i + l' + \rho'}$$

and

$$\frac{K''}{\mu'} = L + \rho' + \rho'' + \frac{(l' + \rho')(l'' + \rho'')}{i},$$

we have

$$D'_1 = \frac{e'}{e''} \cdot \frac{i - l'' + \rho''}{i} \cdot \frac{\lambda' v' \sqrt{b'}}{R' \sqrt{a'}} \delta K',$$

$$D'_2 = \frac{e'}{e''} \cdot \frac{i + l'' + \rho''}{i} \cdot \frac{1}{R'} \delta R',$$

$$D'_3 = \frac{e'}{e''} \left\{ L + \rho' + \rho'' + \frac{(l' + \rho')(l'' + \rho'')}{i} \right\} \frac{v' \sqrt{b'}}{R' \sqrt{a'}} \delta \lambda'.$$

* When investigating the minimum absolute magnitude of S, the terms could be taken without an accent, because S contains only terms belonging to the same station. When investigating D this cannot be done, as D contains also terms belonging to the other station.

put

$$\frac{e'}{e''} = s,$$

$$\frac{i + l'' + \rho''}{i} = J,$$

$$L + \rho' + \rho'' + \frac{(l' + \rho')(l'' + \rho'')}{i} = T,$$

and

$$\frac{1}{R' \sqrt{\frac{a'}{b'}}} = \frac{1}{\psi'};$$

$$\therefore D'_1 = sJ\lambda'v' \cdot \frac{1}{\psi'} \delta K',$$

$$D'_2 = sJ \frac{1}{R'} \delta R',$$

$$D'_3 = sv' \frac{T}{\psi'} \delta \lambda'.$$

Now keeping s , J , λ' , and v' constant, D'_1 becomes smaller for any given $\delta K'$ the larger ψ' is selected; while D'_3 becomes smaller for any given $\delta \lambda'$ the smaller $\frac{T}{\psi'}$ is selected, and D'_2 becomes smaller the larger R' is chosen.

Now $\psi' = R' \sqrt{\frac{a'}{b'}}$ has a maximum for $a' = b'$; for

$$R' = b' + a' + d' = b' + \gamma';$$

and putting $\gamma' = b't'$, we have

$$\psi' = (1 + t') \sqrt{a'b'},$$

which, for $a' + b'$ and t' constant, has clearly a maximum for $a' = b'$. This proceeding is right, because we take b' as the original variable, and vary a' and γ' simultaneously with b' in order to keep t' and $a' + b'$ constant; while J and s are independent of a' , b' , and γ' .

In order to be sure that $a' = b'$ makes also D'_3 a minimum, we must show that T keeps constant, *i. e.* ρ' keeps constant when a' varies. But $\rho' = a' + f'$; thus we have only to consider f' simultaneously variable with a' , equal and opposite to the variation of a , which is allowed. Therefore the condition $a' = b'$ makes undoubtedly the disturbances D'_1 and D'_3 minima; while the disturbance D'_2 , which contains R' in the denominator only, is not affected by this relation, but depends on the absolute value of b' only, which should be chosen as large as possible.

$a=b$ is therefore the *second* regularity-condition, the fulfilment of which makes the relative disturbance of balance by a variation of K and λ as small as possible.

Substituting now $a'=b'$ in the expression of the D disturbances, and remembering that

$$R'=K',$$

we get

$$D'_1 = s\lambda'v' \frac{J}{K'} \delta K',$$

$$D'_2 = s \frac{J}{K'} \delta R',$$

$$D'_3 = sv' \frac{T}{K'} \delta \lambda'.$$

Thus D'_1 and D'_2 , for constant s , λ' , and v' , become smaller the smaller $\frac{J}{K'}$ is, while D'_3 becomes smaller the smaller $\frac{T}{K'}$ is.

Now remembering that

$$J = \frac{i+l''+\rho''}{i},$$

$$K' = \frac{(l''+\rho'')(i+l'+\rho') + i(l'+\rho')}{i+l''+\rho''},$$

and

$$T = L + \rho' + \rho'' + \frac{(l'+\rho')(l''+\rho'')}{i},$$

$$\therefore \frac{J}{K'} = \frac{(i+l''+\rho'')^2}{i\{(l''+\rho'')(i+l'+\rho') + i(l'+\rho')\}},$$

$$\frac{T}{K'} = \frac{i+l''+\rho''}{i} = \frac{1}{\mu''}.$$

For a tolerably good line $l''+\rho''$ as well as $l'+\rho'$ can be taken as small in comparison with i ; hence, approximately,

$$\frac{J}{K'} = \frac{1}{l'+l''+\rho'+\rho''} = \frac{1}{L+\rho'+\rho''},$$

and

$$\frac{T}{K'} = 1,$$

from which it follows that also for the compensation method ρ' and ρ'' should be selected as large as possible.

But $\rho=a+f$ does not give a condition besides that we know we should select a and f absolutely *not* small.

Further, we see that the disturbance D'_3 has v' for its factor, while D'_1 has $\lambda'v'$ for its factor.

Hence for a given $\lambda'v'$, the best will be to make v' as small as possible.

The regularity of the signals is therefore obtained if we fulfil the following conditions in either station :—

$$R=K,$$

$$a=b,$$

$$\rho \quad \text{as large as possible,}$$

$$v \quad \text{as small as possible.}$$

Knowing this, we may now consider that balance in either station is rigidly obtained, or that

$$R\sqrt{a}-K\lambda v\sqrt{b}=0;$$

but

$$R=K,$$

and

$$a=b;$$

we have

$$\lambda v=1.$$

The absolute value of a may now be determined by considering that it is advisable to produce the signals in either station in the most economical manner.

Maximum Magnetic Moment.

We have

$$P'=\frac{e''}{a''+f''+c''}\mu'q'\sqrt{a'},$$

$$P''=\frac{e'}{a'+f'+c'}\mu''q''\sqrt{a''}.$$

But

$$\frac{\mu'}{a''+f''+c''}=\frac{\mu''}{a'+f'+c'}=\frac{i}{Q},$$

where

$$Q=i(L+\rho'+\rho'')+(l'+\rho')(l''+\rho'');$$

$$\therefore P=P'+P''=i\frac{e''q'\sqrt{a'}+e'q''\sqrt{a''}}{Q},$$

which has a maximum for a' and a'' taken as independent variables.

If we, for instances, take $i=\infty$, then

$$P=\frac{eq\sqrt{a}}{L+2(a+f)};$$

$$\therefore a=\frac{L}{2}+f \text{ for a perfect line ;}$$

and by inference

$$\left. \begin{aligned} a' &= \frac{L'}{2} + f', \\ a'' &= \frac{L''}{2} + f'', \end{aligned} \right\} \text{approximately.}$$

Now we can decide on the method to be adopted for readjusting balance. On account of the regularity-condition $R=K$, and as both undergo variation, especially K , we are obliged to adjust balance in the compensation branch by varying the resistance d , and leave the coils or their armatures stationary.

Thus the general solution of the first problem for the compensation method is :

1. *Readjustment of balance is to be effected by a variation of resistance in the compensation circuit, and not by a movement of the coils or their armatures. By this adjustment R is kept equal to K permanently, no matter in which branch the variation takes place.*

$$\begin{aligned} 2. \quad f &= w + \beta; \\ a &= b = \frac{L}{2} + f; \\ v\lambda &= 1, \end{aligned}$$

v as small as possible and λ as large as possible.

β is known from the number and nature of the single cells of which the battery has to consist to produce through the given line (connected up in a circuit like fig. 3) single signals with sufficient strength.

w is known from the absolute largest variation β may undergo in time; hence f is determined, and therefore also a and b .

Determination of λ and v .

We know that $\lambda v = 1$, and, further, that $\lambda = \frac{E}{e}$ should be selected as large as possible or v as small as possible; but otherwise it appears that no fixed values for λ and v can be ascertained. If we, however, consider the nature of the variations of R and K which may disturb the balance, viz. those variations of R and K which are due to unavoidable decrease of the internal resistance of the two batteries by the working currents, it will be seen that a best value of λ does exist, and that therefore v also becomes fixed.

Suppose that at a certain moment

$$R = K$$

is rigidly fulfilled; and remembering that

$$R = b + d + \alpha,$$

$$K = 2(\alpha + f) + L$$

(for a perfect line, i. e. $i = \infty$), and that, further,

$$a = b$$

and

$$f = w + \beta,$$

we have

$$d + \alpha = a + 2w + 2\beta + L.$$

Now in this equation suppose every thing constant except α and β , the internal resistance of the two batteries E and e respectively. Hence, if we could achieve that

$$\delta\alpha = 2\delta\beta \text{ invariably,}$$

the variation of the internal resistance of the two batteries would not disturb the equation $R = K$, and therefore also not affect the balance. With absolute certainty we cannot fulfil this desirable relation between the two variations; but with some probability we may. For it is well known that the internal resistance of a galvanic battery decreases in time by the current passing through the battery. Hence, if we suppose that the two batteries consist of identical cells (equal in nature, size, and internal resistance), we may say that the variation of the internal resistance of a single cell by the unit current in the unit of time is the same for both the batteries. Further, if we make the other not improbable supposition that the variation at any one time is proportional to the current which passes at that time, we have

$$\delta\alpha = \epsilon E \cdot \frac{E}{R + \delta R} \phi^{(t)} = \epsilon \frac{E^2}{R} \phi^{(t)}$$

and

$$2\delta\beta = \epsilon e \cdot \frac{e}{K + \delta K} \phi^{(t)} = \epsilon \frac{e^2}{K} \phi^{(t)},$$

where ϵ is the variation of the internal resistance of a single cell in unit of time by unit of current, $\phi^{(t)}$ a certain unknown function of the time which, as the two batteries are working simultaneously, is not required to be known.

Hence, from

$$\delta\alpha = 2\delta\beta$$

and

$$K = R,$$

it follows that

$$\lambda = \frac{E}{e} = \sqrt{2}$$

and

$$v = \frac{r}{q} = \sqrt{\frac{1}{2}}.$$

These values of λ and v bring the compensation method,

with respect to regularity of working, as close to the differential method as is possible for us to do ; for the disturbance of balance in the sending-station by the steady decrease of the internal resistance of the two batteries has now been *probably* eliminated, which defect is excluded from the other two methods by their own nature. There are then remaining only those variations of the battery-resistance which do not follow the law of steady decrease, but which are more accidental, and make therefore the compensation method still inferior to either the differential or bridge method.

$$\text{Physical meaning of } v = \sqrt{\frac{1}{2}}.$$

It has been proved that balance in each station is to be established by adjusting resistance, and *not* by a movement of the coils or their armatures. Hence it will be practical and convenient to coil the two helices above each other and have them acting on one and the same iron core.

Further, as $v = \frac{r}{q} = \sqrt{\frac{1}{2}}$, it follows that the magnetic action of the a coil must be made greater than that of the b coil. Therefore it will be best to coil the helix b on the top of the helix a .

Further, the magnetic action of a cylindrical coil of resistance a (in Siemens units) can be expressed as follows—

$$m = s \sqrt{a} \sqrt{\frac{A\lambda}{cl}},$$

where A is half the cross section of the coil (cut by a plane through the axis of the coil) expressed in square millimetres ;

λ the absolute conductivity of the wire material ($H_g = 1$ at 0° C.) ;

l the length of an average convolution expressed in metres ;

s the magnetic force exerted by an average convolution of the coil when the unit of current passes ;

c a coefficient representing the manner of coiling.

Hence for the a coil we have

$$m_a = s' \sqrt{a} \sqrt{\frac{A'\lambda'}{c'l'}} = q \sqrt{a},$$

for the b coil

$$m_b = s'' \sqrt{b} \sqrt{\frac{A''\lambda''}{c''l''}} = r \sqrt{b}.$$

Dividing m_b by m_a , and remembering that by condition $a=b$, and that $\lambda'=\lambda''$, $c'=c''$ by necessity, we have

$$v = \frac{r}{q} = \frac{s''}{s'} \sqrt{\frac{A''l'}{A'l''}}.$$

As we have supposed that the magnetic action of any one cylindrical coil is proportional to the magnetic action* of an average convolution, it is also consistent to put $s' = s''$, and we have at last

$$\frac{A''l'}{A'l''} = \frac{1}{2}.$$

If now the two bobbins of the coils a and b are taken of equal length, and if the thickness of the a coil be d' , the thickness of the b coil d'' , and the diameter of the iron core $2r$, we have

$$\frac{A''}{A'} = \frac{d''}{d'},$$

$$l' = (2r + d')\pi,$$

$$l'' = \{2(r + d') + d''\}\pi;$$

$$\therefore (4r + d')d'' = 2d'(r + d').$$

This equation fixes the relative dimensions of the two bobbins and their cores in order to have $v = \sqrt{\frac{1}{2}}$.

Suppose, for instance, we make $d' = d''$ arbitrarily †, we get $2r = d$; and from it can be easily calculated that the diameter of the wire of the b coil should be *about 19 per cent. larger* than that of the a coil. The absolute diameter of the wire depends of course on the absolute dimensions of the bobbins, and on the resistance of the line for which the instrument is to be

* Lenz and Jacobi have experimentally proved that, within certain limits, the magnetic force exerted by a convolution on its centre (iron core) is almost independent of the diameter of the convolution. These limits are generally fulfilled in telegraph-construction. Hence the magnetic action of a coil can be put proportional to the magnetic action of *one* convolution. Theoretically this can of course not be true; for the magnetic force exerted by a convolution necessarily extends on both sides of the plane in which the convolution is situated. Therefore the wider a convolution is, the less of its total force exerted will be made use of for producing magnetism in the iron core, and consequently the force exerted by a convolution on its centre must decrease with the diameter of the convolution. It appears, however, that this decrease is exceedingly slow, and in the present investigation it is considered unnecessary to be taken into account.

† I have not been able to find anywhere a definite law which connects the diameter of a coil with the diameter of the core acted upon. In Siemens's relay, an instrument so well considered in all its details of construction, the diameter of the coil is about three times the diameter of the core. In the absence of any thing else on the subject I thought myself justified in using this proportion. Hence the substitution of $d' = d''$, which gives $d = 2r$, or total diameter of the a coil equal to three times the diameter of the iron core.

used. But this question, although of practical importance, has nothing to do with the Theory of Duplex Telegraphy. This settles the solution of the first problem of the compensation method.

Other methods.—There have been suggested from time to time many other methods of duplex working. On a closer examination it will, however, be found that, as a general rule, they do not differ essentially from the three fundamental methods treated of. I shall therefore dispense with the labour of investigating these derived methods.

In case it should be thought necessary to investigate them, no difficulties ought to be met with, if only the general plan of attacking duplex problems be remembered, viz.:—draw the diagram of the method in its most general form; develop the forces p , P , and Q ; from these three forces determine the functions S and D ; find the relations which must hold between the different variables (resistances and electromotive forces) of which the system consists, in order to make S and D simultaneous minima; consider the question of *immediate balance* which determines also the best mode of adjusting balance; consider that the movement of the key must not alter the complex resistance of the station to which the key belongs, *i. e.* that the working of the key must not affect the balance of the distant station; determine the absolute values of the different variables when balance is rigidly fulfilled by considering the question of economy, *i. e.* establish the relations for maxima currents and maxima magnetic moments. Any variables which should then be left indeterminate must be fixed by secondary considerations and by certain practical conditions.

Before comparing quantitatively the efficiency of the three fundamental methods treated of, it is required to solve two questions, viz. the electromotive force required for each duplex method, and the absolute size of the increments of the adjustable resistance.

[To be continued.]

LXIII. *Remarks on the Discovery of Gallium.*

By D. MENDELEJEFF*.

IN 1869 I announced the following periodic law:—"The properties of the simple bodies, as also the properties and constitution of their combinations, are periodic functions of the atomic weights of the elements."

Among the different applications of that law I shall cite only the following:—

1. This law constitutes the basis of a complete systematic classification of the elements.

* Translated from the *Comptes Rendus* by M. M. Pattison Muir.

Series.	1st group.	2nd group.	3rd group.	4th group.	5th group.	6th group.	7th group.	8th group.
	R_2O	RO	R_2O_3	RH_4 RO_2	RH_3 R_2O_5	RH_2 RO_3	RH R_2O_7	(R_2H) R_2O_4
1.	H=1							
2.	Li=7	Be=9	B=11	C=12	N=14	O=16	F=19	
3.	Na=23	Mg=24	Al=27	Si=28	P=31	S=32	Cl=35	
4.	K=39	Ca=40	?=44	Ti=48	V=51	Cr=52	Mn=55	Fe=56, Co=59, Ni=59, Cu=63.
5.	(Cu=63)	Zn=65	?=68	?=72	As=75	Se=78	Br=80	
6.	Rb=85	Sr=87	Yt=88	Zr=90	Nb=94	Mo=96	?=100	Ru=104, Rh=104, Pd=106, Ag=108.
7.	(Ag=108)	Cd=112	In=113	Sn=118	Sb=122	Te=125	I=127	
8.	Cs=133	Ba=137	Di=138	Ce=140	"	"	"	Os=195, Ir=197, Pt=198, Au=199.
9.	"	"	"	"	"	"	"	"
10.	"	"	Er=178	?La=180	Ta=182	W=184	?=190	"
11.	(Au=199)	Hg=200	Tl=204	Pb=207	Bi=208	"	"	"
12.	"	"	"	Th=231	"	U=240	"	"

2. The periodic law demands a change in the atomic weights of certain metals which have not as yet been sufficiently studied.

Atomic weights of the metals and formulæ of their oxides.			
	Old numbers.	Numbers proposed by me.	
Indium	75 InO	113 In ₂ O ₃	{ The specific heat, determined by Bunsen, and likewise by me, has confirmed the change (<i>Bull. de l'Acad. St. Pétersbourg</i> , vol. viii. p. 45).
Uranium.....	120 U ₂ O ₃	240 UO ₃	
Cerium	92 CeO Ce ₃ O ₄	138 Ce ₂ O ₃ CeO ₂	{ Rammelsberg and Roscoe have accepted the change (<i>Ber. der deut. chem. Ges.</i> vol. v. p. 1003, and vol. vii. p. 1131).
Thorium	116 ThO	232 ThO ₂	
Yttrium	60 YO	90 Y ₂ O ₃	{ The specific heat of the metal (<i>Bull. de l'Acad. St. Pétersbourg</i> , vol. viii. p. 45) and the constitution of its salts (<i>Lieb. Ann.</i> vol. clxviii. p. 46) appear to confirm my proposal.
Erbium	114 ErO	171 Er ₂ O ₃	
Didymium? or } Lanthanum? }	about 92 RO	138 R ₂ O ₃	

3. The periodic law indicates vacancies in the classificatory scheme of the known elements, and enables us to predict the properties of elements, as yet undiscovered, and of their compounds. For example, there are two vacancies in the third and fourth groups respectively of the fifth series : I have called the unknown elements which should fill these vacancies *Eka aluminium*, El, and *Eka silicium*, Es.

The properties of eka aluminium, according to the periodic law, should be as follows. Atomic weight about 68 ; formula of its oxide El₂O₃ ; its salts have the general formula ElX₃. Thus, for example, the chloride of eka aluminium will be ElCl₃ ; on analysis it will yield 39 per cent. of metal and 61 of chlorine ; and it will be more volatile than ZnCl₂.

The sulphide, El₂S₃, or the oxysulphide, El₂(S, O)₃, ought to be precipitable by sulphuretted hydrogen and to be insoluble in ammonium sulphide. The metal will be easily obtained by

reduction ; its density will be about 5·9, its atomic volume about 11·5 ; it will be fusible at a somewhat low temperature.

It will not oxidize in air ; it will decompose water at a red heat. The pure metal will not be easily attacked by acids or by alkalies. The atomic volume of the oxide, $\text{El}_2 \text{O}_3$, will be about 5·5 : this oxide will be soluble in the stronger acids ; it will form an amorphous hydrate insoluble in water, but dissolved by acids and by alkalies.

The oxide of eka aluminium will form neutral and basic salts, $\text{El}_2 (\text{OH} \cdot \text{X})_6$, but not acid salts ; the alum, $\text{El K} (\text{SO}_4)_2 12 \text{H}_2\text{O}$, will be more soluble and less crystallizable than the corresponding aluminium salt. The basic properties of $\text{El}_2 \text{O}_3$ will be more distinct than those of $\text{Al}_2 \text{O}_3$, but not so marked as those of ZnO ; it will belong to that group of oxides which are precipitable by barium carbonate. The volatility and other properties of the salts of eka aluminium stand midway between those of the salts of aluminium and of indium : it is probable that the metal in question will be discovered by means of spectral analysis, as was the case with indium and thallium.

The characteristics of eka aluminium were deduced from a consideration of the place occupied by this hypothetical element in the periodic system :—

Series.	2nd group.	3rd group.	4th group.	5th group.
3. . .	Mg	Al	Si	P
5. . .	Zn	El	Es	As
7. . .	Cd	In	Sn	Sb

It ought to be remarked that, previous to the discovery of the periodic law, it was not possible to predict the existence or to foretell the properties of undiscovered elements.

M. Lecoq de Boisbaudran has discovered, by means of spectral analysis, the existence of a new element in the blende of Pierrefitte (Pyrenees) ; to this element he has given the name of gallium. The manner of its discovery, the process for its separation (precipitation by sulphuretted hydrogen before zinc), and certain of its properties (precipitation by barium carbonate, solubility of the hydrate in ammonia, degree of volatility, &c.), make it probable that the new metal is no other than eka aluminium.

If subsequent researches confirm the identity of the properties of gallium with those which I have pointed out as belonging to eka aluminium, the discovery of this element will furnish an interesting example of the utility of the periodic law. Let us hope that the discovery of eka silicium, $\text{Es}=72$, EsO_2 , the probable properties of which I have detailed in Liebig's *Annalen*, vol. viii. (Suppl.) p. 171, will not be long de-

layed. It should be searched for more especially as associated with arsenic and with titanium.

Note by the Translator.

Since the publication of M. Mendelejeff's paper the discoverer of gallium has made known the results of further researches upon the properties of this metal and of its compound. He finds that the salts of gallium have most probably the general formula Ga X_3 , that the oxide is best represented by the formula Ga_2O_3 , that ammonia precipitates solutions of the chloride and sulphate of gallium probably with formation of a hydrate, that this precipitate is soluble in acid and in alkali, that gallic ammonio-alum is a crystalline salt almost certainly isomorphous with ordinary alum. M. Lecoq de Boisbaudran also finds that metallic gallium may be readily obtained by the electrolysis of an ammoniacal solution of the sulphate, that the metal is not oxidized in air at 200° , and that it readily decomposes acidulated water, especially at high temperatures. In these respects gallium appears to correspond with the hypothetical eka aluminium of M. Mendelejeff. Determinations of the atomic weight and of the specific heat of the new metal will be awaited with great interest.

LXIV. *On Unitation*.—VI. *Some of the Applications and Developments of the General Formula (continued)*. By W. H. WALENN, *Mem. Phys. Soc.*

[Continued from S. 4, vol. 1. p. 527.]

18. **T**HE decimal equivalents of all reciprocals of the form $\frac{1}{10^a - 1}$ have the terminal figure of their period equal to 1. This is manifest from the fact that 1 must be the remainder when the period recommences; for the condition of recurrence is that the dividend is the same as that which, in the operation of division, commences the work, and this is uniformly, in the case of reciprocals, 10 or some power of 10. In the case of reciprocals of the form $\frac{1}{10^a - 1}$, the product of the figure in the quotient which is the terminal figure of the period must have its function $U_{10}f$ (f being the product) equal to 9, because this product has always to be subtracted from a number which has its unitate to the base 10 equal to 10. But the denominator itself of the reciprocal is the only multiple which has its unitate to the base 10 equal to 9; therefore the said terminal figure is 1.

19. To put a portion of the theorem illustrated in art. 17 *

* *Phil. Mag.* [IV.] vol. 1. p. 526.

into definite language :—The decimal equivalents of reciprocals of the form $\frac{1}{10a-1}$ may be constructed, without the aid of division, from the right-hand end of the period, solely by means of the multiplier a , this multiplier being used to obtain the next figure towards the left from the previous one as a multiplicand, and adding in the tens' digit that may be carried from the previous product.

Expanding the formula $\frac{1}{10a-1}$, by division, it becomes

$$\frac{1}{10a} + \frac{1}{(10a)^2} + \frac{1}{(10a)^3} + \dots + \frac{1}{(10a)^{n-2}} + \frac{1}{(10a)^{n-1}} + \frac{1}{(10a)^n},$$

n being the number of figures in the recurring period of the decimal. By art. 18, it has been proved that the last term, namely $\frac{1}{(10a)^n}$, is 1. If $\frac{1}{(10a)^n} = 1$, then

$$\frac{1}{(10a)^{n-1}} = \frac{10a}{(10a)^n} = 10a,$$

and the series (commencing from the right-hand term) becomes

$$\dots + (10a)^4 + (10a)^3 + (10a)^2 + 10a + 1.$$

The above theorem is thereby proved in relation to reciprocals that have two figures in the denominator, a being the multiplier therein mentioned. By exactly similar reasoning, the theorem may be extended to reciprocals having more than two figures in the denominator.

20. In regard to the remainders to the division that forms the reciprocal $\frac{1}{10a-1}$, by taking an example, the relation of the unitates (to the base 10) of these remainders to the figures in the quotient will be distinctly seen to be as follows :—

$$39)1\cdot000000(\cdot02564\dot{1}$$

$$\begin{array}{r} 78 \\ 220 \\ 195 \\ \hline 250 \\ 234 \\ \hline 160 \\ 156 \\ \hline 40 \\ 9 \\ \hline 1 \\ 202 \end{array}$$

That is, $U_{10}r$ (r being the remainders obtained by subtracting the products, in the usual way of division, from the remainders previously obtained) is always the same as the figure in the quotient which belongs to the previous product. This is a consequence of $U_{10}9n$ being the series 9, 8, 7, 6, 5, 4, 3, 2, 1—that is, complements to 10 of their multipliers n .

21. According to another way of regarding like operations to that set forth in art. 20, to have 1 remainder, 39 must have been subtracted from 40, giving 1 for the last figure in the quotient; and the 4 requires 6 to be the unite (to the base 10) of the product that gives the next figure, to the left hand, in the quotient. The only product of 39 that gives that unite is 156 ($=39.4$), giving 4 for the penultimate figure in the quotient, and so on towards the left hand for the other figures in the quotient by simple inspection of the function $U_{10}39n$ (n being the digital multipliers of 39); thus division is performed, by means of unitation, in the opposite direction to that of the ordinary operation.

22. Reciprocals of the form $\frac{1}{10n-3}$ have 7 for the last figure of their period; for the function $\frac{1}{U_{10}^{-1}7} = 7 \cdot \frac{1}{U_{10}^{-1}9}$ (the symbol $U_{\delta}^{-1}x$ being read “the number whose unite to the base δ is equal to x ”), and (art. 18) $\frac{1}{U_{10}^{-1}9}$ has 1 for the terminal figure of its period. The multipliers to construct these reciprocals from right to left are comprehended in the arithmetical series 5, 12, 19, 26, &c., the corresponding denominators being 7, 17, 27, 37, &c. That this is the series is evident (art. 19) from the equations, $\frac{1}{7} = 7 \cdot \frac{1}{49} \dots$ having multiplier 4 + 1, $\frac{1}{17} = 7 \cdot \frac{1}{119} \dots$ having multiplier 11 + 1, $\frac{1}{27} = 7 \cdot \frac{1}{189} \dots$ having multiplier 18 + 1, and so on.

Taking $U_{10}r$ to mean the same as put forward in reference to reciprocals of the form $\frac{1}{10a-1}$ (art. 20), but having relation only to reciprocals of the form $\frac{1}{10n-3}$, $U_{10}r$ may be made to yield the figures of the quotient in the case $\frac{1}{10n-3}$ by multiplication by 7; this is a manifest result from the equation

$$\frac{1}{U_{10}^{-1}7} = 7 \cdot \frac{1}{U_{10}^{-1}9}.$$

The series for performing the division to obtain the period by means of unitation (art. 21) is that of $U_{10} 7n = 7, 4, 1, 8, 5, 2, 9, 6, 3, 10$.

23. Similarly for reciprocals of the form $\frac{1}{10n-7}$. Since

$\frac{1}{U_{10}^{-1} 3} = 3 \cdot \frac{1}{U_{10}^{-1} 9}$, these reciprocals have 3 for the last figure of their period. The multipliers to construct these reciprocals from right to left are comprehended in the arithmetical series 1, 4, 7, 10, &c., the corresponding denominators being 3, 13, 23, 33, &c. In this case the function $U_{10} r$ yields the figures of the quotient by multiplication by 3, and the series for performing the division inversely is that of $U_{10} 3n = 3, 6, 9, 2, 5, 8, 1, 4, 7, 10$.

Also by similar processes to the above, reciprocals of the form $\frac{1}{10n-9}$ are found to have 9 for the last figure of their period. The multipliers to construct them from right to left are comprehended in the arithmetical series 10, 19, 28, 37, &c., the corresponding denominators being 11, 21, 31, 41, &c. The function $U_{10} r$ yields the figures of the quotient by multiplication by 9; and the series for performing the division inversely is that of $U_{10} 11n = 1, 2, 3, 4, 5, 6, 7, 8, 9, 10$.

The other forms of reciprocals are $\frac{1}{10n-2}$, $\frac{1}{10n-4}$, $\frac{1}{10n-5}$, $\frac{1}{10n-6}$, and $\frac{1}{10n-8}$. When reduced to decimals, these are either finite, or they can be derived from the form $\frac{1}{10n-1}$ by multiplication or division. For instance, $\frac{1}{64} = .015625$, and is finite; $\frac{1}{54} = \frac{1}{2 \cdot 27} = \frac{7}{2} \cdot \frac{1}{189} = 3\frac{1}{2} \cdot \frac{1}{189}$; $\frac{1}{72} = \frac{1}{8 \cdot 9} = \frac{1}{8} \cdot \frac{1}{9}$.

All other decimal equivalents that are derived from fractions, being multiples of reciprocals, can be obtained by multiplication in connexion with one of the above methods.

The employment of unitation to investigate the constitution and properties of decimal equivalents derived from fractions shows the analytical power of unitation, and points out another method of checking calculations into which recurring decimals enter.

74 Brecknock Road, N.,
April 1876.

[To be continued.]

LXV. *Notices respecting New Books.*

'Reliquiæ Aquitanicæ,' being contributions to the *Archæology and Palæontology of Périgord, &c.* By EDOUARD LARTET and HENRY CHRISTY. Edited by Professor T. RUPERT JONES, F.R.S., F.G.S. 4to, Illustrated, 1865-75. Williams and Norgate, London.

OF the many discoveries relating to Archaic man and his surroundings which have of late years been made and described, none have been more fully and carefully examined than the Troglodytic relics from the caves in the valley of the Dordogne. In none have the finds been more complete, more varied, or more interesting; in none has the enthusiastic scientific interest of an able series of observers been more fully evidenced.

The work has passed through many hands. First, it was the intention of the late Mr. Henry Christy, with the hearty cooperation of M. Edouard Lartet, to produce a work descriptive of the results of their personal labours, which should be useful to the Anthropologist and Archæologist alike. But the former gentleman's lamented death hindered the continued exhaustive examination of the caves; and though his executors were fully desirous of carrying out his wishes in the question, still his loss to some extent prevented the work reaching those dimensions which its original promoter ardently desired. Nor was this the only check the history received. M. Edouard Lartet, on whom the prosecution of the work now mainly devolved, passed away, and with him the opportunity for the completion of a careful description of the mammalian remains of which he was so able a judge.

Notwithstanding these unavoidable drawbacks, the work was continued by Professor T. Rupert Jones, F.R.S., to whose careful and painstaking editing the completion of Mr. Christy's plan is due; and the book as it stands is a noble monument of MM. Christy and Lartet. Its general arrangement, whereby not merely the relics of the Cave Folk have been described, but their uses have been suggested, comparisons made with the habits and customs of known savage races, and monographs by able hands added when necessary, making the history of these singular people as complete as conjecture based on sound reasoning could make it.

The ancient province of Aquitania, originally lying between the Garonne, the sea, and the Pyrenees, was finally extended to the Loire, and subdivided into three districts, with Avaricum (Bourges), Burdigala (Bordeaux), and Elusa (Eauze) as their capitals.

The explorations examined in the book refer to only a portion of this area, viz. that part of Périgord in which lies the department of Dordogne.

Following the railway from Orléans, through Limoges and Périgueux, the valley of the Vézère, a tributary of the Dordogne, which again is an affluent of the Garonne on its right bank, is finally reached; and here the hitherto undulating country undergoes a marked change. The valley is bounded by steep escarpments, in

which the great flutings and projecting cornices of rock show how the eroding action of weather has taken advantage of the relative hardness of the limestone bands. These beds are very nearly horizontal, but dip slightly towards the north-west; and the irregular action of the weathering or destroying agent has therefore produced at many places, both at high and low levels, caves and rock shelters in which the ancient dwellers of the valley lived. Thus the cave at Les Eyzies is 100 feet above the river-level, and Le Moustier 90 feet, while others again are but little above the level of the extraordinary inundations of the Vézère, though now, owing to the accumulation of a talus of limestone débris, the entrances are often completely hidden, as in the Cro-Magnon cave, the discovery of which was accidentally made during the construction of a railway approach.

The excavations and discoveries were made at six places: at Laugerie Haute, Laugerie Basse, Le Moustier, Gorge d'Enfer, and La Madelaine were rock shelters; at Le Moustier, Les Eyzies, Gorge d'Enfer, and Cro-Magnon were caves; and in all these numerous relics of human occupation and quantities of mammalian and other bones were found buried and concealed, sometimes under successive layers of stalagmite, sometimes under masses of earth and detritus, resulting from the destruction of the rocky roof of these ancient dwellings.

The method whereby these hollows thus became places of bone-deposit had been long a subject for conjecture. Some attributed it to the action of wild beasts who made these caves their lairs; others deemed the action of torrent-water washing down the remains of animals and filling these hollows a sufficient cause; but that Man has had more to do with this infilling of the cave-stuff is fully proved by the evidences of human work and art in the relics he left behind. The first suggestion, that they were Hyæna-lairs, may be dismissed at once, as no gnawed bones were found among the débris; and this probably further indicates that the ancient inhabitants had some means of closing the entrances against such intruders.

The relics described and pictured are chiefly of stone, bone, and antler, with some of ivory, and a few shells and other ornaments. The particular prehistoric age or horizon to which these may be referred can be more or less easily determined. There are three great periods into which prehistoric time is usually divided:—the surface-period, where implements are found associated with tombs and places of sepulture; and here the relative age of each lies within the province of the archæologist. The Cave-period, previous to the construction of regular habitations for the living or tombs for the dead, offers with the implements only bones of animals whereby the palæontologist can estimate their antiquity. Then the drift-period carries us further back still in time; and here the ruder implements are rarely definitely associated with bones, there are no other human relics to guide us, and the determination of their age therefore rests only with the geologist: many of the older

caves, however, belong to this era, as may be determined by the nature of their contents.

Even these great periods have their minor subdivisions. With the first and last we have nothing to do; but the cave-dwellers of this area have a somewhat definite boundary. They were Reindeer-hunters. This is the true basis on which the age of these Troglodytes can be formed. In the "drift" the rough flint tools are associated with the Mammoth and Rhinoceros, the Horse and the Ox, but the Reindeer is rare. In some caves the Reindeer bones predominate; but the implements are chipped, not polished, there are no traces of spinning or the use of vegetable fibre, and, though the Horse and Aurochs remain, the Mammoth is becoming rare. In the kitchen-middens of Denmark the Reindeer has disappeared, and the dog has become man's companion; while in the Swiss dwellings*, and later still in the Cromlechs, polished stone, spindle whorls, and, finally, metal ornaments and weapons show the gradual advance of art and civilization.

And yet far back in time as these ancient hunters of Périgord must have lived, the 'Reliquiæ Aquitanicæ' show most clearly that they had attained a knowledge of art and even decoration of no mean description, considering the primitive nature of the implements they had in use. Thus the deposits consisted of accumulations of broken bones, pebbles from the neighbouring stream, countless chips of flint, with the cores from which they were struck, implements of bone and horn, arrow-heads and needles, all buried in a débris mixed with charcoal to a depth of even 10 feet in some cases, over an area 60 or 70 feet long.

There are some slight differences in the nature of the deposits in the various caves, though that is not in itself of necessity an evidence of difference of age; for that other animals than the Reindeer were coeval with it at the period of occupation of the Vézère valley, is evidenced by the excellent sketches and sculptures from the bones at Les Eyzies, Laugerie Basse, and La Madelaine, which must have been drawn from an intimate knowledge of the animals depicted.

Thus, though the Reindeer, Horse, and Aurochs are the chief sources of food-supply, there is less of the first-mentioned at Laugerie Haute and Le Moustier cave; and at the latter are bones of the *Elephas primigenius*, the Hyæna, and *Felis spelæa*. Still the Troglodytes did not fare badly in quantity or quality; for bones of Ibex and Chamois, Birds and Salmon lie among the refuse heaps; and inasmuch as many of the smaller bones still remain articulated, it is evident that there was not such lack of food as to render close eating necessary. Nor were they ignorant of commerce, art, and decoration. Rock-crystal, which does not occur in the neighbourhood, and fossil shells from the Faluns of Touraine, one hundred miles away, indicate that they had some communication with the outer world; and at Cro-Magnon the presence of sea

* The Reindeer formerly existed in the neighbourhood, as the drawing of a Reindeer from the Kesslerloch cave indicates.

shells, common on the shores of La Charente, point out that this family at least had ascended the valley of the Dordogne after a sojourn on the Atlantic coast.

Personal decoration is instanced by finding pieces of "soft red hæmatite covered with scratches," indicating how a red powder was produced "which mixed with grease would furnish as good means of personal adornment as is employed by many of the Indians of the present day;" and pierced teeth of various animals, sometimes marked with incised markings and carved, together with shells drilled for suspension, may well have furnished necklaces or ornaments which can easily find their analogue among Esquimaux or Indians of modern times.

But it is in the actual implements of war, chase, and domestic life that the researches have been most definite. The materials used were bone and stone, the latter generally flint, the former of different kinds.

The flint implements varied much in style and character. Some were finely and delicately chipped to form arrow-heads and oval lances of true Scandinavian type, as at Laugerie Basse. Others, as at Le Moustier, more nearly approach the drift-type of St. Acheul in roughness of make, being mere blocks of flint, one side of which has been reduced by chipping to a cutting edge, forming either a tool for splitting open the marrow bones (which among all the hearth-stuff of the caves are found so broken in profusion), or as hand weapons for close combat in a *mêlée*. Manifestly what could split the skull of an animal carried to the cave for the purpose of extracting the brain, could with equal facility be made to crack a human skull of similar density. Long even flakes of flint with their extremities chipped into an oval form, thumb-flints, fitting conveniently to the grasp, were used evidently in dressing skins, or as rasps and scrapers of wood and bone, though possibly those which bear marks of wear and minute crushing or chipping along the side, and hence turned "side scrapers," were better for the latter purpose. The flakes therefore bear marks of wear in three ways:—first at the end, which is rounded artificially, the other being left pointed; next at the side, the rest of the flake being untouched; and lastly at the point, on one or both sides. This latter form may have been produced in two ways—either by scraping a round substance like an arrow-shaft, in which case it becomes a more or less complete notch, or as a drill used for piercing the eyes of the needles, or other holes in wood and other materials.

These all have their representatives in implements of existing savage tribes; and much care has been taken in the book to institute these comparisons, and so build up an hypothesis based on what is actually known. The Esquimaux lydite scraper and the New-Caledonian obsidian lance-head shown at p. 14, can be exactly paired by implements from the cave shelters of Périgord. The side scrapers are more common in the Le Moustier caves, the other rock shelters providing articles of a higher degree of finish. In some instances the rough chipping has been extended all round the

flake, especially in the thicker kinds, producing a form exactly similar to the modern briquet or "strike-a-light" used in France and elsewhere. These always fit easily to the left hand, and are fit for use with pyrites and tinder even now; and here again a full description of the modern method of using these implements is given, to furnish argument for the assumption that these ancient chipped flints were made for a specific purpose.

As before remarked, the vast majority of the worked stones are of flint, which, owing to their conchoidal fracture, can readily be flaked or knapped so as to produce a cutting edge, especially if the chips be removed from alternate sides of the implement, thus producing that wavy regular edge which is the best test of intentional work. But a considerable number of rounded excavation of varying depth on one side. These may have been in some instances mortars, as suggested, in which materials could be rubbed or pounded; but there may be another use to which these stones could be put. In one of the excellent papers detailing the method of procedure adopted by Esquimaux and others for chipping the edges of the arrow-heads, Capt. Sir. E. Belcher says that the workmen, "selecting a log of wood in which a spoon-shaped cavity was cut, placed the splinter to be worked over it, and by pressing gently along the margin vertically (with an implement of hard reindeer horn), first on one side and then on the other as one would set a saw," produced finally the desired serrate-edged arrow-head. Whether this hard material would answer the same purpose as the softer block of wood is a matter of conjecture; but as many of the hollows seem too small for mortars, it is possible they may have had some such use.

Le Moustier has been the only cave producing ovate implements of the type common in the valley-gravel of the Somme, and having the same glaze; but in the other caves more delicately chipped lance-heads, flakes of a finer character, and even semilunar neatly dressed implements of Danish type are common in the caves of Laugerie Haute and the Gorge d'Enfer.

But it is in the implements of bone that the discoveries have been peculiarly rich, both from the artistic and finished nature of the specimens, and from the excellent drawings which ornament some of them.

The materials are usually reindeer horn, though other bone material is occasionally employed; and the implements are of various kinds, from the delicate bone needle with oval eye (which in some cases has been renewed lower down when the first eye was broken) to long pointed dart-heads and barbed harpoons. These latter are precisely similar to those in use among the Esquimaux; and at the lower end the bone has been so pared away as to leave a projecting boss or ring, which may have been designed both to prevent the head from being thrust too far into the shaft at the risk of breaking or splitting it with the force of the blow,

and as the place of attachment of one end of a lanyard of sinew thong, or grass string, the other being attached to the shaft itself, so that the latter, as in the arctic regions, might form a buoy if the harpoon head were buried in the fish. That such may have been their use is suggested by the sketches of fishes which ornament some of the weapons.

But the most interesting articles in the collection are the long implements of reindeer-horn pierced with several large equidistant holes and ornamented by linear carving or rows of animals, and a long dagger poignard of deer-horn, the handle of which is artistically carved into the shape of a reindeer. The former may have been arrow-straighteners, game clubs (Pogamagans), or even batons of office. It is difficult to assign any other use to them. Those that are much perforated were evidently not weapons; and the care taken in the ornamentation would seem to indicate that they were either worn as ornaments or carried in the hand.

The needles are carefully made and scraped or polished to a smooth surface, either with the flakes ("side scrapers," which would account for the large number of used specimens), or by rubbing on a stone; and indeed several stones are figured the long narrow furrows of which are exactly suitable for such a purpose.

The sketches and markings are most various and suggestive. Many of them are probably owner-marks, or mere attempts at decoration in an idle moment; and others may from their regularity be tallies or the marks of gambling-tools. A most interesting paper in support of this view, and furnishing numerous authentic examples of markings for a similar purpose, is contributed by the Editor, which throws considerable light on the possible intention of similar obscure or in themselves meaningless marks.

The animals sketched on the various fragments afford the most reliable means of discovering some of the habits of this ancient race. Ibex, deer and reindeer, ponies with heavy heads (the truthfulness of which has since been proved by the discovery of equine skulls identically similar in character), carp and pike, the human figure, a gloved hand resembling the embroidered mitten of the Esquimaux, and, lastly, the great shaggy mammoth, *Elephas primigenius*, are depicted, generally with distinctness, frequently with a boldness and accuracy of outline that is all the more extraordinary inasmuch as the sole graver the ancient artist had was a flint flake, his only panel the generally curved surface of a piece of horn.

The drawing of the Mammoth is executed on a portion of the outside layer of the tusk of an elephant, probably of the same species, the attitude (that of an animal drinking at a spring) natural and easy; and in the opinion of Mr. Henry Woodward, F.R.S., the sketch, rude as it necessarily is, "was the result of a *life* study of the animal, and is consequently of the highest importance, as attesting the actual presence of the living Mammoth in France when the Caves of Périgord were occupied by Man."

The early excavations were unattended by any definite discovery of human remains, though finally portions of skeletons were found in

the stations at La Madelaine and Laugerie Basse, the bones of which were in the same state of preservation as those of the reindeer associated with them, and were imbedded in the same medley of chipped flints and animal bones of which the bed of the cave is composed; but though the nature of the remains was undoubted, great reserve was exhibited in theorizing on the subject until the important discovery of the Cro-Magnon cave placed it beyond a doubt that the Cave Folk buried their dead in places that had been habitations.

The Cro-Magnon cave, or rather rock shelter, was discovered during the formation of a railway, which led to the removal of the talus which hid the mouth of the cave, and the consequent unexpected discovery of another "station." Several successive layers of hearth-stuff and worked flints, covered by intermediate layers of *débris* which had fallen, in weathering, from the roof, indicated that the shelter was not always permanently occupied, and that finally, when the height of the cave had been considerably lessened by the accumulation of rubbish and detritus, the back part was used as a place of sepulture, where the skeletons of an old man, a woman, an infant not fully developed, and other bones, surrounded by shells pierced for necklaces, ivory pendants, worked reindeer-horn, and chipped flints, were exhumed.

The skulls were all of the dolichocephalic type; and the skeletons were those of persons of large stature, their platycnemic bones characterizing them as a peculiar race existing here in the Reindeer period.

One peculiarity connected with the woman's remains is that the skull bears the mark of a violent death, there being a clear oval hole in the left temple which corresponds in shape and size with that which could be made by one of the flint lance-heads found in the caves. Whether this was the result of an accident or to an injudicious and excessive exercise of marital authority in this ancient dwelling is of course a matter of conjecture; but Dr. Pruner-Bey's conclusion as to the appearance of the members of this family deserves quotation.

"As to his exterior, a heavy frame was managed by a powerful muscular apparatus, which has left its traces in the hollows and ridges of the bones. A robust but flattish foot bore the body, and was fitted for running by its elongated heel. Of a sombre aspect, with an imposing stature, and conscious of his strength, ignorant of moderating his passions by a cultivated *morale*, he could be violent and turn against the weaker sex the weapon intended to kill his prey. Indeed, whether we regard the wound inflicted on the woman described above as the result of a family quarrel, or of a combat between hostile tribes, it must be noted that as yet nothing parallel has been found among the human remains dating from that distant period."

It is exceedingly difficult to assign a definite age to these ancient remains. Cro-Magnon, from the presence of bones of Bear, Mammoth, great Cave-lion, and Reindeer, has apparently some

of the oldest deposits of the series ; and the successive evidences of habitation shown by the several layers of hearth-stuff, separated by varying thicknesses of débris (the formation of which was, if modern evidence be worth any thing, exceedingly slow), would serve to carry the period of sepulture back to a very distant period. The additional evidence of the heavy talus, 4 to 6 mètres thick, which had gradually formed against the mouth of the rock shelter must not be forgotten.

Le Moustier may even be older, judging from the rudeness of the tools and their similarity to the drift-type, as well as to the nature of the animal remains, Mammoth, Hyæna, and *Felis spelæa*, with few relics of the Reindeer. It is possible that additional evidence of antiquity is found in the higher level of this cave above the bottom of the valley.

The others, judging from the associated remains, are more of the same age, and were occupied when the larger animals were rarer and the reindeer in large numbers had taken their place. These latter were evidently plentiful, as the bones belong to animals of all ages, judging from the horns.

It is still more difficult to fix the date of occupation of the valley at this period. Though reference is made to the reindeer by Sallust as existing in the German forests, there is no *historical* evidence whatever of its having lived in Southern Gaul. The climate of that time, though the classic writers speak of winters of greater extremes of cold than are known now, would still be unsuitable to the reindeer ; and it is probable that Aquitania, being nearer the shores of the Mediterranean, was more densely occupied and more disforested than those portions of Northern Germany of which the classical author speaks.

We must go back still further in time for the Reindeer epoch of Aquitaine, to a period when the glaciers were larger and more extensive, and when Western Europe was more influenced by the glacial sea. In no other way can the presence of vast herds of reindeer be accounted for. There were no lofty mountains near, to which in the summer heat, when they are peculiarly susceptible to being troubled by flies, they might retreat ; nor were ranges to the north to which they might go, and, in passing near the Vézère during their migrations, come within reach of the reindeer-hunters. Their presence in the neighbourhood must have been constant ; for the Cave-folk evidently drew constant and sufficient supplies of food from the reindeer herds, and they would not have been able to kill and *store* enough for their wants during the intervals which elapsed between such assumed migrations. Savage races, as a rule, follow up the animals they hunt for food, and make no permanent habitations unless the former inhabit the neighbourhood in sufficient numbers. The selection of caves and shelters with a southern aspect, the constant occupation evidenced by the quantity of material found in the caves, all show that the men and animals were inhabitants of the same district, and that the climate, therefore, must have been suited to such a case.

The 'Reliquiæ Aquitanicæ' furnish the best arguments in support of this view in the excellent monograph on the Reindeer by Mr. Alex. Anderson; and the other essays on the customs of existing Indian races, the nature of implements used by them, and the character and suitability of flint for implement-making are most valuable, not merely to arrive at a just conclusion of the use of the various relics in the caves of the Dordogne, but also as data whence the value and character of other discoveries of a similar nature may be deduced. The illustrations are executed with extreme care and beauty, and the descriptions which accompany the numerous lithographs are most valuable and suggestive.

In the concluding words of the preface, "it must be noticed that the resolution of Mr. Henry Christy's executors, desirous of fully carrying out the last wishes of their brother, to give every assistance in producing the book in the style he contemplated, has been amply and generously fulfilled; and we believe the hope formerly expressed has also been fulfilled—namely, that, supported by the goodwill and aid of friends, this useful work, though not so largely comprehensive as was once intended, will be a fit and lasting memorial of him whose energy, liberality, and love of science originated its design, collected its materials, and furnished the means for its completion." The errata have been carefully attended to in the supplemental notes, which contain also many additional remarks and references. A copious index adds to the value of the book, making it readily available for students.

LXVI. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 330.]

March 22, 1876.—Professor P. Martin Duncan, M.B., F.R.S., President, in the Chair.

1. "ON the Triassic Strata which are exposed in the Cliff Sections near Sidmouth, and a note on the occurrence of an Ossiferous Zone containing Bones of a *Labyrinthodon*." By H. J. Johnston Lavis, Esq., F.G.S.

2. "On the Posterior Portion of a Lower Jaw of *Labyrinthodon* (*L. Lavis*) from the Trias of Sidmouth." By Harry Govier Seeley, Esq., F.L.S., F.G.S., Professor of Physical Geography in Bedford College, London.

3. "On the Discovery of *Melonites* in Britain." By Walter Keeping, Esq. Communicated by Prof. T. McKenny Hughes, F.G.S.

4. "Note on the Phosphates of the Laurentian and Cambrian Rocks of Canada." By Principal Dawson, LL.D., F.R.S., F.G.S.

The author described the mode of occurrence of phosphatic deposits in various localities in Canada. Dark phosphatic nodules, containing fragments of *Lingula*, abound in the Chazy formation at Allumette Island, Grenville, Hawkesbury, and Lochiel. Similar

nodules occur in the Graptolite shales of the Quebec group at Point Levis, and in limestones and conglomerates of the Lower Potsdam at Rivière Ouelle, Kamouraska, and elsewhere on the Lower St. Lawrence; these deposits also contain small phosphatic tubes resembling *Serpulites*. The Acadian or Menevian group near St. John, New Brunswick, contains layers of calcareous sandstone blackened with phosphatic matter, consisting of shells and fragments of *Lingula*. The author described the general character of the phosphatic nodules examined by him at Kamouraska, and gave the results of analyses made of others from various localities, which furnished from 36.38 to 55.65 per cent. of phosphate of lime. A tube from Rivière Ouelle gave 67.53 per cent. The author accepted Dr. Hunt's view of the coprolitic nature of the nodules, and inclined to extend this interpretation to the tubes. The animals producing the coprolites could not be thought to be vegetable-eaters; and he remarked that the animals inhabiting the primordial seas employed phosphate of lime in the formation of their hard parts, as had been shown to be the case with *Lingula*, *Conularia*, and the Crustaceans. The shells of genus *Hyolithes* also contain a considerable portion of phosphate of lime. Hence the carnivorous animals of the Cambrian seas would probably produce phosphatic coprolites.

With regard to the Laurentian apatite deposits, the author stated that they to a great extent form beds interstratified with the other members of the series, chiefly in the upper part of the Lower Laurentian above the *Eozoon*-limestones. The mineral often forms compact beds with little foreign matter, sometimes several feet thick, but varying in this respect. Thin layers of apatite sometimes occur in the lines of bedding of the rock. Occasionally disseminated crystals are found throughout thick beds of limestone, and even in beds of magnetite. The veins of apatite are found in irregular fissures; and as they are found principally in the same parts of the seams which contain the beds, the author regarded them as of secondary origin. The Laurentian apatite presents a perfectly crystalline texture; and the containing strata are highly metamorphosed. The author's arguments in favour of its organic origin are derived from the supposed organic origin of the iron-ores of the Laurentian, from the existence of *Eozoon*, from the want of organic structure in the Silurian deposit described by Mr. D. C. Davies, and the presence of associated graphite in both cases, from the character of the Acadian linguliferous sandstone, which might by metamorphism furnish a pyroxenite rock with masses of apatite like those of the Laurentian series, and from the prevalence of animals with phosphatic crusts in the Primordial age, and the probability that this occurred also in the earlier Laurentian. The position of the phosphatic deposits above the horizon of *Eozoon* is also adduced by the author as adding probability to the existence of organic agencies at the time of their formation.

April 5th, 1876.—Prof. P. Martin Duncan, M.B., F.R.S.,
President, in the Chair.

1. "The Bone-caves of Creswell Crags."—Second paper. By the Rev. J. Magens Mello, M.A., F.G.S.

2. "On the Mammalia and Traces of Man found in the Robin-Hood Cave." By W. Boyd Dawkins, Esq., M.A., F.R.S., F.G.S., F.S.A., Professor of Geology and Palæontology in the Owens College, Manchester.

3. "Notes on the Gravels, Sands, and other Superficial Deposits in the neighbourhood of Newton-Abbot." By Horace B. Woodward, Esq., F.G.S.

The writer pointed out that most of the deposits termed Upper Greensand in the immediate neighbourhood of Newton-Abbot were in reality intercalated with coarse gravel-beds containing, among others, fragments of greensand, chert, and chalk flint. He considered that the only traces of Greensand *in situ* were probably on the summit of Milber Down and east of Combe Farm, deposits which were identified by Mr. Godwin-Austen. But he could not agree in the identification of Greensand at other localities in the Bovey valley, considering the few fossils found to have been derived, and, with much other material, to have been evidently due to the denudation of Chalk and Greensand. He pointed out the geographical distribution of these beds of sand and gravel, which extend from the hill-tops bordering the Bovey valley to near the bottom of the valley, but do not descend into any outlying valleys. He likewise alluded to the peculiar dip into the valley which affects these beds in several places, and observed that sometimes they rested on the Bovey Clays and Lignites. He thought that in their method of formation some connexion might be traced with somewhat similar deposits on the Haldon and Black-Down Hills.

He pointed out that the "Head" at the bottom of the valley was sometimes not to be distinguished from the older gravels, from which, however, it was largely derived. He alluded to the discovery of bones, a bronze spear-head, and a wooden doll or idol in this deposit—observing that they indicated the rapid accumulation of gravel, and that this indication was one out of many that might be given that our modern river-gravels are to a great extent made up of older gravels.

In conclusion the writer alluded to some of the deposits now forming on the margin of the Teign estuary, and which are identical in character with the Triassic breccia.

4. "On certain Alluvial Deposits associated with the Plymouth Limestone." By R. N. Worth, Esq., F.G.S.

The author adduced certain deposits found in fissures and caverns of the Plymouth Limestone as furnishing evidence in opposition to the views advocated by Mr. Belt in his paper on the Drifts of Devon and Cornwall*. The best examples occur at Plymouth Hoe, where the chief deposit fills a large "pocket" in the limestone, and consists (beneath the turf) of a bed of clayey soil, containing pebbles and small boulders, beneath which are patches of white and red clay containing a few pebbles and overlying a large quantity of siliceous sand. Similar but slightly varying deposits not unfrequently occur in association with the Limestone; and these are regarded by the

* See Quart. Journ. Geol. Soc. vol. xxxii. p. 80.

author as the remains of considerable deposits which once occupied large areas in the valleys of South Devon; and if they are not the low-land gravels of Mr. Belt, the latter are not represented in the district. The author states that there is evidence of the contemporaneity of these deposits with those of the Oreston caves; and he adds that they furnish no proof of cataclysmal action, but of orderly deposition, the bulk of the pebbles and gravels being inland, nearer the source of the *débris*, and further off the sands and clays, in fairly regular succession. The author further explains the presence in Cornwall of stanniferous gravels only in valleys opening to the south, by reference to the position of the watershed in that county, which has only two rivers running to the north, whilst on the south-east rivers abound.

April 26, 1876.—Prof. P. Martin Duncan, M.B., F.R.S., President, in the Chair.

1. A Translation of a Notice, by Capt. Miaulis of the Greek Royal Navy, of the occurrence of a Submarine Crater within the Harbour of Karavosserra, in the Gulf of Arta. Communicated by the Secretary of State for Foreign Affairs.

2. "The Physical History of the Dee, Wales." By Prof. A. C. Ramsay, LL.D., F.R.S., V.P.G.S.

The author stated that he regarded the valley of the Dee as mainly preglacial throughout, and sketched the physical history of the region through which it runs. The Silurian rocks were much disturbed and denuded before and during the Carboniferous period, and the Carboniferous Limestone was deposited very unconformably on the upturned edges of both Lower and Upper Silurian strata, and once spread all over the region, probably overlain by the Millstone grit and Coal-measures, as now in the east of Denbighshire and Flintshire. The region was again disturbed and elevated during the formation of the Permian deposits; and then by subaerial denudation a great part of the Carboniferous series was removed down to the old plain of denudation of the Silurian rocks, the surface of which thus probably stood higher than it does at present, being in the midst of a broad continental area. From a consideration of the conditions of deposition of the Mesozoic and Tertiary formations the author concluded that, from the beginning of the Permian to that of the Glacial epoch, the higher ground of Wales was land well raised above the sea, except perhaps during the deposition of the Chalk, and that during all this period it was exposed to the influence of subaerial agents of denudation. He indicated the conditions of elevation of the old tableland of Carboniferous rocks, and showed that it had probably a slope towards the east and north-east to the extent of about 23 feet in a mile. The drainage of this land then flowed in an easterly and north-easterly direction along the earliest channel of the Dee, which would be at an elevation from 1300 to 1400 feet higher than the present channel.

During the Glacial epoch ice-action deepened and more or less modified the existing channel, and scooped out the basin of Bala

Lake, which was not previously in existence. The general results of this investigation are as follows:—After the last important disturbance of the præ-Permian rocks, North Wales was carved slowly and by subaerial agencies into its present mountainous form chiefly between Permian and Preglacial times. The work of the glaciers of the latter period somewhat deepened, widened, smoothed, and striated the minor outlines of the mountains and valleys, and excavated many rock-bound lake-basins, but did not effect any great changes in the contours of the country. A minor submergence of part of Britain during part of the Glacial epoch produced no important effects on the large outlines of the rocky scenery; and the effects of subaerial waste subsequent to the Glacial epoch have been comparatively small.

3. "On the Ancient Volcano of the District of Schemnitz, Hungary." By John W. Judd, Esq., F.G.S.

The old volcanoes of Hungary have long been known to present some very interesting illustrations of the relations between the igneous rocks erupted at the surface, and those which have consolidated at a considerable depth beneath it. The district in which these phenomena can be best studied is that of Schemnitz; but although this area has been very carefully mapped and explored by a number of able investigators, the greatest diversities of opinion still exist concerning the relations of certain of the rock-masses exposed within it.

Over an area nearly fifty miles in diameter enormous accumulations of *andesite* and *quartz-andesite* lavas and agglomerates have been erupted, these now forming a group of mountains rising from 3000 to 4000 feet above the sea-level, and culminating in a great ring of precipitous heights overlooking a depressed central area of oval form, the site of the famous mining towns of Schemnitz, Kremnitz, and Königsberg. In the midst of this depressed central area there occurs a considerable development of *rhyolitic* lavas and tuffs, and more scattered outbursts of *basalt*. From the magnificent floras associated with the various volcanic tuffs, we know that the andesitic rocks were erupted during the earlier portion of the Upper Miocene period, and the rhyolitic towards its close, while the basalts are probably of as late date as the Pliocene.

Besides the rhyolites and basalts, however, there are certain other rocks exposed in the central area of the Schemnitz district, the relations of which it is very difficult to understand. These consist of (1) strata of Lower Trias and Nummulitic age, through the midst of which the volcanic outbursts have evidently taken place; (2) masses of highly metamorphic rocks, including quartzites, crystalline limestones, various schists, gneiss and aplite; and (3) undoubted eruptive rocks, which have usually been called "syenite and granite," but for which the names of "diorite and quartz-diorite" would perhaps be more appropriate, inasmuch as the prevailing felspar in them is always a plagioclase variety.

By Beudant and other early writers the andesitic lavas were

recognized as volcanic products of a comparatively recent geological period, while the "granite, syenite, and greenstone" were regarded as being of far more ancient date. By Von Pettko, Richthofen, and all the more recent investigators of the district, however, it has been clearly perceived that the "greenstones" are certainly, like the andesites, of Tertiary age; and hence such names as "greenstone-trachyte" and "propylite" have been applied to them. The studies of the author of the present memoir, both in the field and in the cabinet, have led him to the conclusion that the granitic, porphyritic, and lava rocks—which were formerly called "syenite," "greenstone," and "trachyte" respectively—are all of *similar composition and equivalent age*, and that they differ only in their more or less perfect state of crystallization, the result evidently of variation in the conditions under which they have consolidated. He is further led to regard the metamorphic masses around the several intrusive centres as being not, as has hitherto been maintained, of "Primary" (Devonian or Permian) age, but simply Triassic rocks affected by local or contact metamorphism.

The real structure of the great Schemnitz volcano was first recognized by Von Pettko in 1848, though this author erroneously regarded it as presenting an example of a "crater of elevation." The history of the formation and destruction of this volcano is now shown to be as follows:—After some small and scattered outbursts of rocks of acid composition towards the close of the Oligocene period, the grand eruptions of andesitic lavas of the Miocene began, through the agency of which a volcano of larger dimensions than Etna was gradually built up, by both central and lateral eruptions. In the midst of this volcano a crater of enormous dimensions was formed, doubtless by some great paroxysmal outbreak; and by the subsequent subsidence of the mountain the sea gained access to, and by denudation greatly enlarged the area of, this "caldera." Then in the central lagoon of the caldera a number of minor eruptions, first of acid and then of basic rocks, took place; and the volcano, which at this period of its history must have closely resembled the existing island of Santorin, was again upheaved from beneath the sea, and exposed to the wasting effects of subaerial denudation. The gradual decline of the volcanic forces in the district was marked, as is usually the case, by the appearance of hot and mineral springs, discharges of gas, occasional earthquakes, &c.

While affording such remarkable examples of the perfect transition between the so-called plutonic and the volcanic classes of rocks, and of the phenomena of contact metamorphism, the granitic masses of the Schemnitz district are without question truly *intrusive*; and a careful study of them lends no support whatever to the hypothesis that such rocks may be formed by the extreme metamorphism of sediments *in situ*. There is the most complete proof that in the Schemnitz district the formation of true mineral veins, containing gold, silver, and other metals, has taken place within the most recent geological periods—in some cases, indeed, at a later date than the Pliocene.

LXVII. *Intelligence and Miscellaneous Articles.*

ON THE EFFECT OF THIN PLATES OF IRON USED AS ARMATURES TO ELECTROMAGNETS. BY JOHN TROWBRIDGE, S.D., HARVARD COLLEGE, U.S.

IN a paper presented to the Academy, April 13, 1875, I showed that the application of armatures to two straight electromagnets, which formed the primary circuit of a Ruhmkorff coil, more than doubled the strength of the induction-current produced by breaking the primary circuit. When, however, the circuit of the secondary coil was not closed, and a spark was allowed to jump across the interval between its poles, the striking-distance of the spark, and its power to charge a condenser, did not seem to be notably increased by the application of armatures to the electromagnets of the primary circuit. My experiments at that time were made with solid iron cores; and I now resume these experiments with bundles of fine iron wires in place of the solid iron cores. The mechanical difficulty of making the ends of the bundles of fine wires constituting the cores plane surfaces was overcome by dipping them in melted solder, and then filing the surfaces. In this way I had no difficulty in applying the armatures so that they should lie upon a plane surface.

The resistance of each of the two induction-coils covering the two straight electromagnets was 6000 ohms, and that of each of the straight electromagnets $\cdot 34$ of an ohm. The diameter of the bundles of fine iron wires constituting the cores was 5 centims., and the length of the electromagnets was 28 centims. Condensers of various sizes were placed in the primary circuit: the results given in this paper were obtained by the use of a condenser of about one farad. The method of experimenting was to charge a condenser of $\frac{1}{3}$ of a farad by means of a spark one millimetre in length, and then to discharge this condenser through a galvanometer. If we express the quantity of electricity received by the condenser by Q , the electromotive force and the capacity of the condenser by E and C , we have $Q = EC$.

We also have $Q = \frac{2nt}{\pi} \sin \frac{1}{2} \phi$, where n is the reduction factor of the galvanometer, t the time of vibration of the magnet, and ϕ the arc through which it swings under the effect of the charge. Knowing the reduction factor of my galvanometer, I had thus the means of reducing my results to absolute measure. But I speedily found that the relative results obtained by the proportions

$$Q : Q' = \sin \frac{1}{2} \phi : \sin \frac{1}{2} \phi' = E : E'$$

would present the points of this investigation in a manner as valuable as if the results had been reduced to absolute magnetic measure.

My first experiments were made with solid armatures.

TABLE I.

Without armatures.	With armatures.
80	90
70	80
90	100
60	70
70	85
80	90
Mean ... 75	86

In this Table the numbers are the deflections of the reflecting galvanometer expressed in millimetres. In this case the gain by the use of the armatures was trifling, being only about 14 per cent. These results were obtained by charging the condenser of $\frac{1}{3}$ of a farad by sparks one millimetre in length.

On a closed secondary circuit, however, a gain of one hundred per cent. was clearly shown in the strength of the induced current produced by breaking the primary circuit. The question of how to make this great increase in the strength of the induced current by the employment of armatures manifest in the spark became an interesting one. It seemed at first as if the application of armatures, by maintaining the temporary magnetization of the iron cores, would be detrimental rather than otherwise.

I next tried the effect of bundles of thin iron plates, which were placed as armatures upon both poles of the electromagnets, thus making a magnet of a horseshoe form. On charging the condenser I found a very great increase in quantity, which was manifested by the swing of the galvanometer-needle, the indications being entirely off the scale. Table II. shows the results obtained by the use of iron plates $\frac{1}{24}$ of an inch in thickness, twenty in number, constituting each armature.

TABLE II.

Without plates.	With plates.
80	400
70	380
90	370
60	400
70	370
80	400
Mean ... 75	386.6

Here a gain of four hundred per cent. was manifested by the use of the thin plates.

The next step was to ascertain how many plates were necessary to obtain the maximum effect. The difficulty of obtaining plates

of the same homogeneity made it impossible to obtain smooth curves. To this difficulty was added that of breaking the primary circuit in a regular manner.

If the results of Table III. be plotted, it will be seen that the increase within small limits is very nearly proportional to the number of thin plates (which were $\frac{1}{24}$ of an inch in thickness).

TABLE III.

No. of plates.	Deflections of galvanometer.	No. of plates.	Deflections of galvanometer.
1	11	6	15
2	12	7	15.5
3	13	8	18
4	13	9	18
5	14	10	18.5

On increasing the number of plates, a point was reached where there was no additional effect. The best result was obtained where the mass of the armatures was approximately equal to that of the cores of the electromagnets. Plates of $\frac{1}{32}$ of an inch thickness were also used; but no advantage resulted in their employment, over those of $\frac{1}{28}$ of an inch. It would seem that the thin plates followed the same law as the bundle of fine iron wires which constitute the cores of induction-coils of the present day, and that only a moderate degree of discontinuity in the mass of iron submitted to magnetic influence is necessary to prevent the formation of currents of induction, which prolong the magnetism of the cores and prevent the quick demagnetization necessary to produce intense currents of induction. The effect of insulating the thin plates with thin dielectrics, like paper, was also tried, with no gain in effect. There appeared to be a slight gain by placing the plates edgewise on the poles of the electromagnets, instead of allowing them to repose on their flat sides. This was doubtless due to better contact of the metallic surfaces.

Since the above results proved conclusively a very great gain in quantity and electromotive force by the application of thin plates as armatures, I next measured the striking-distance of the spark. Table IV. gives the results, which are the mean of many trials.

TABLE IV.

Without armatures.	With armatures.
15 centims.	32 centims.
14 "	30 "
15 "	32 "
Mean 14.6	31.3

A curious fact came up in this connexion. The lengthening of the spark was not shown when the spark leaped directly between

the poles of the induction-coil; the increase in quantity and electromotive force was only made manifest to the eye by the employment of condensers in the secondary circuit. The results in Table IV. were obtained by the employment of a Leyden jar of large capacity. The increase in the quantity and electromotive force was not only shown by the increased length of the spark, but also by its increase in volume and its loud snap. The spark consisted of a thick central bolt, surrounded by curious thin detached sparks. An attempt was made to measure the increase of light in Geissler tubes by Vierodt's photometric apparatus; but it was found too inexact for this purpose—if, indeed, there was any increase of light, which certainly remains to be proved. I know of no results which bear upon the relation of the increase of light to the increase of electromotive force of the induction-spark.

Without condensers in the secondary circuit, however, the increased electromotive force of the spark was shown by its greater constancy in leaping over a given resistance of air.

The results of this investigation can be thus summed up:—

1. The application of thin plates of iron as armatures to two straight electromagnets increases between four and five times the strength of the spark produced by the surrounding secondary coils.

2. The length of the spark is doubled, which is only shown by the use of a condenser in the secondary circuit.

3. The results show that it would be more economical to construct induction-coils consisting of two straight electromagnets constituting the primary circuit, and two fine coils constituting the secondary circuit, with the use of thin plates of iron as armatures to the electromagnets, than to distribute the same amount of wire on one straight electromagnet as in the common form of Ruhmkorff coil.—*From the Proceedings of the American Academy of Arts and Sciences*, Feb. 9, 1876.

“FELDSPAR” AND “FELDSTONE” VERSUS “FELSPAR” AND “FELSTONE.”

Of the two modes of spelling the above mineralogical names, the former is followed by the Germans of the present day, and the latter by the English.

“Felspar” (that is, the word without the *d*) occurs in the works of Brooke and Miller (1852), Greg and Lettsom (1858), Nicol (1849), Thomson (1836), Phillips (1816–1837), Allan (1814–1834), Brooke (1823), Aikin (1807, 1815), Jameson (1804–1820), Kirwan (1784, 1794); so that if good names can make any orthography right, that of “felspar” is abundantly sustained.

We find, however, among British mineralogists, Mr. John Williams, in his ‘Mineral Kingdom’ (1810), using the German form “feldspar;” and so also Mawes, in his ‘Descriptive Catalogue’ (1821), and Townson, in his ‘Philosophy of Mineralogy’ (1798); and Nicholson, in his ‘Dictionary of Chemistry,’ like Magellan in his translation of Cronstedt’s ‘Mineralogy’ (1788),

adopts essentially the same, though taking for the first syllable the Swedish spelling "felt-spar," as given in Wallerius (1743). Again, Haidinger's English translation of Mohs (Edinburgh, 1825) has "feldspar." Further, Jameson in his edition of 1804 uses "felt-spar" with a caveat; for he adds in a note at the bottom of the page, "More properly *feldspar*."

Kirwan appears to have decided the question in favour of "felspar" for all his successors (or at least set the current in that direction) by means of a piece of bad etymology. He says, "This name seems to be derived from *Fels*, "a rock," it being commonly found in granite, and not from *Feld*, "a field;" and hence I write it thus, *felspar*." Kirwan recognized the Germanic origin of the word. But he must have reached his decision without taking German works for authority.

Among the Germans "feldspath" (meaning *field-spar*) ("felt-spat" or "fält-spat" of the Swedes) is the only form that has ever been used. Wallerius, in 1747, nearly forty years before Kirwan issued his first edition, and Cronstedt, in 1758, nearly thirty years, used the term "felt-spat" or "fält-spat" in their treatises; and Grimm, in his German Dictionary, the latest and best, has "feldspath," and not "fels-spath." There can hence be no dispute as to the true spelling or derivation. "Feldspar" therefore is the right word among those who speak English.

French mineralogists have conformed, with a rare exception, to the German style, they retaining the *d*. Thus it is with Des Cloizeaux (1862), Dufrénoy (1844-1860), Huot (1841), Beudant (1824, 1832), Brochant (1808), Haüy (1801-1822), Romé de Lisle (1783), Démeeste (1779). But Brongniart (1807) thought, like Kirwan, that "feldspath" must be a corruption of the "mot originaire *felspath*;" and hence, as he says, he "restored to the mineral its first name." Later French mineralogists have not followed Brongniart's example.

In the United States, "feldspar," except in sporadic cases (a consequence of British example), has been the adopted form. It is the word used in the works of Dana (1837-1868), Shepard (1832-1857), Robinson (1825), and Cleveland (1816-1822)*.

Thus British mineralogists are at present alone in using "felspar." There is nothing in favour of the word, unless it be its fewer letters by one, and its smoother pronunciation; and such a reason for change, if acted on, would make havoc of good English. Being wrong, and "feldspar" being right, its rejection should encounter no impediment in the fact of long national usage. Science is not national; and only by the change can uniformity of nomenclature be secured.

The case between "feldstone" and "felstone" is very similar to that between "feldspar" and "felspar." The original German word is "feld-stein," one long since introduced into the science, and not *fels-stein*. *Fels-stein* would be as bad in German as *rock-stone* (its signification) in English. The Germans cannot adopt *fels-stein*,

* Phillips's 'Mineralogy,' in its republications in the United States, has retained the English orthography.

since it is an absurd compound ; and the half English word “fel-stone” is no less objectionable. Neither could ever find place in any European work on lithology.

Felsite is an old word in the science, used by mineralogists of all lands.

ON THE DISPLACEMENT OF THE LINES IN THE SPECTRA OF THE STARS, PRODUCED BY THEIR MOTION IN SPACE. BY FATHER SECCHI.

Rome, March 25, 1876.

It is now generally admitted, in accordance with the ideas put forth by Doppler in 1842, that the displacement of a luminous point which is moving from or approaching towards the observer produces an alteration in the length of the light-wave. From this principle, combined with those of spectral analysis, it follows that the lines of a substance existing in a star in motion will be displaced in relation to those obtained with the same substance on the earth. The amount of the displacement will depend on the velocity of the star in the direction of the line of sight of the observer. Objections to this theory have not been wanting ; but it appears to have been generally received*, and confirmed by the observation of curvatures in the lines of the solar protuberances.

To ascertain the rigorous accuracy of this theory I made some experiments in 1863 ; but the results at that time were negative. I attributed the want of success to the inadequacy of my instruments. A little while after, Mr. Huggins resumed the question, and found some sensible displacements, of which he gave an account to the Royal Society of London ; but, fearing nevertheless an illusion produced by the manner of illuminating the slit, he made some changes in his procedure and examined several stars afresh. In 1872 he gave a list of motions judged to be definitive†.

In this interval I was often occupied with the subject, especially in 1868‡, employing sometimes processes of comparison, sometimes absolute methods ; but my results did not always agree with those of the celebrated English spectroscopist. Nevertheless, out of regard for his great authority, and taking into consideration the very perfect means he had at his disposal, I was led to prefer his results, notwithstanding their opposition to my own. Lately M. Vogel, at Bothkamp, and the Greenwich Astronomers having published some researches on the subject, I found in their results some considerable divergences from those of Mr. Huggins ; and I determined to resume the experiments and make a new examination of the question.

In the following Table I sum up the results of a determination, marking with the sign + the cases of increasing, and with the sign — those of diminishing distance :—

* M. Van der Willigen, however, has contested it.

† See ‘Nature,’ vol. vi. p. 135, July 1872.

‡ See *Comptes Rendus de l’Acad. des Sciences*, March 2, 1868.

Stars.	Observers.			
	Huggins.	Greenwich.	Secchi.	Vogel.
Sirius	+	—	—	
α Lyrae	—	—	0	—
α Aquilæ	\pm	—	
Procyon	+	—		
Castor	+	+	0	
Rigel	+	+		
Regulus	+	—	0	
η Ursæ Majoris...	+	—	—	
α Pegasi	—	—		
α Cygni	—	—		
α Coronæ	+	—		
α Andromedæ ...	—	—		
Arcturus	—	—	0	

It is seen that accordance exists in several instances, but there are numerous contradictions. We recognize moreover by other signs that a cause of error exists as yet unknown, and that the methods of investigation require to be carefully reviewed.

Several facts are at once very striking: (1) nearly all the observations at Greenwich give negative results*, positive results being the exception; (2) the results on different days are not only very discordant as to magnitude, but sometimes even opposite; (3) Mr. Huggins's observations give very similar results for a great number of stars; (4) his results for the comet Coggia are not in accordance with the motion of that comet as known from other sources; (5) the mean values assigned by the different observers are extremely different.

Surprised at these irregularities, I put to myself the question, *Can there be, either in the mode of observing, or in the instruments, a systematic cause of error, producing the displacement of the line, unknown to the observer?* In order to assure myself, I made the following experiments, endeavouring to avoid the conditions which were suspected to be faulty, and to vary as much as possible the other conditions of observation.

First, to illuminate the slit, I placed the Geissler tube simply in front of the objective at a distance of about 5 decims., arranging it so that the capillary tube, about 6 centims. in length, was exactly parallel to the diameter of the objective and to the slit of the spectroscope: the light was sufficiently bright in the ocular of the spectroscope to permit the lines to be seen distinctly. The spectroscope (a direct-vision one) was formed of a Merz prism composed of five strongly dispersing prisms. In the solar spectrum the lines D of sodium were, with this apparatus, separated by double their breadth. To dilate transversely the spectrum of the star, an achromatic cylindrical lens was placed near the eyepiece, inside the little analyzing telescope.

The equatorial being directed to the star Sirius under favourable atmospheric conditions, we were able to see clearly the dark line F of the star simultaneously and in the same field with the hydrogen-

* See 'Monthly Notices,' vol. xxxvi. p. 30 *et seq.*

line $H\beta$. The bright line of the gas was projected outside the dark line of the star, over its border on the side towards the red: the amount of this superposition could not be estimated exactly; for the stellar line (or, rather, band) is very broad, and stumped at its edge, as we have ascertained from the commencement of our researches. We are surprised that observers do not dwell upon this aspect of most of the stellar hydrogen-lines. We shall be pardoned for noting all these details, considering the importance of the consequences which flow from them.

This result was, in the main, the same as that which we had already obtained, especially in 1869, and published in our memoirs *Sur les Spectres Solaires* in 1872, p. 44. The star seemed, then, to have its undulations shortened, and, consequently, to be approaching us, as they found at Greenwich, contrarily to Mr. Huggins.

Not trusting my own estimation, I had the observation made by my assistants, all of them accustomed to these measurements. Observation always gave the same result when the telescope was carried by the clock, and when moreover an assistant was at the searcher to keep it upon a fixed point corresponding to the slit of the spectroscope; but if the clock stopped, or if the assistant deranged the position of the star, the bright line was seen accidentally to place itself on the other side or in perfect coincidence with the dark line. This induced us to repeat more carefully the observation the following day, in order to discover the cause of these variations.

In this second series the results were at first similar to those of the preceding day; but on abandoning the use of the clock the line appeared to take its place on one side or the other, according as the star was struck or carried away on one side or the other with respect to the axis of the telescope. The clock was then again applied, when the bright line appeared on the side towards the red; and after the spectroscope was turned on its axis 180° , the line was situated on the other side for certain observers, remaining on the former side for others.

After producing again and again for a long time these different changes of position, we acquired the conviction that *the line might appear constantly on one side or the other, according to the position of the instrument, without the observer having any sufficiently certain indication to cause him to recognize the illusion of which he was the victim*. This is plainly a very grave conclusion. We do not pretend that observers so skilful have been deceived; but we point out a possible cause of error in this class of observations. We have not yet succeeded in discovering the source of these changes, which may, perhaps, be attributable to a species of parallax, owing to the focus of the star-image not coinciding with the plane of the slit. We confine ourselves to merely indicating the phenomenon which might well have produced an illusion for others as it deceived us. Here the concordance of frequently repeated observations would give no assistance; for, in looking, one always places one's self so as to have the clearest view—that is to say, in such a position that the same error is reproduced.—*Comptes Rendus de l'Académie des Sciences*, April 3, 1876, vol. lxxii. pp. 761–765.

ON A SIMPLE AND VERY ACCURATE METHOD OF TUNING TWO FORKS TO UNISON. BY ROBERT SPICE, F.C.S.

Though the optical method of tuning, of Lissajous, gives good results, I find that two forks thus tuned to unison may be a *fraction* of a vibration out, without in *any way* disturbing the steadiness of the figure.

In the 2nd edition (English) of Tyndall's 'Sound,' in Lecture VII., the author says, "I divide this jar by a vertical diaphragm, and bring one of the forks over one of its halves, and the other fork over the other. The two semicylinders of air produce beats by their interference. On removing the diaphragm, the beats continue as loud as before, one half of the same column of air interfering with the other."

Dr. Tyndall does not, however, mention the fact that precisely the same result would have been obtained if no diaphragm had been employed; yet this is so. When two unison forks are struck on the knee (or by a piece of lead covered with leather) and then held *together* over their proper resonant column, the following phenomena will be observed.

If there is a difference between their rates of several vibrations, there will of course be rapid beats; if the forks are *very* nearly in tune, the beats will succeed each other at long intervals; further, when they are *almost* perfectly in tune, there will not be any beats properly so called, but after the sound of the forks has *nearly* died away, it will rise or swell out again *very slightly*, proving that there had been interference.

Finally, when the forks are absolutely alike, there will be a gradual decrease of sound, down to silence, without any reinforcement at any time.

I find that, to carry out this tuning absolutely, both forks must be at the same temperature; consequently, after using a file on one of them, I place both forks in a vessel of water to equalize their temperatures, wipe them dry, and test them. To show the accuracy of this method, I select the following example:—

A pair of Ut^3 forks (256 vibrations) will sound over a column for about 135 seconds. Suppose that the sound decreases up to the 100th second, and then begins to rise; obviously 100 seconds is the time of *half a beat*, or 200 seconds the beating-time; that is to say, it will have been demonstrated that one of the forks gave $\frac{1}{200}$ of a vibration *per second* more than its fellow.

What has been said of the unison applies to other intervals.

I have recently executed by this method Ut_4 , Ut_5 , Ut_6 , and Ut_7 forks for the physical cabinet of Columbia College.—Silliman's *American Journal*, May 1876.

230 Bridge Street, Brooklyn, Jan., 1876.

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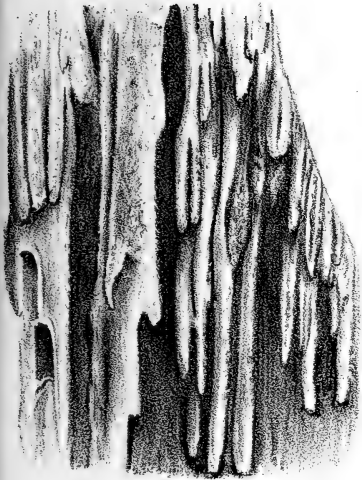
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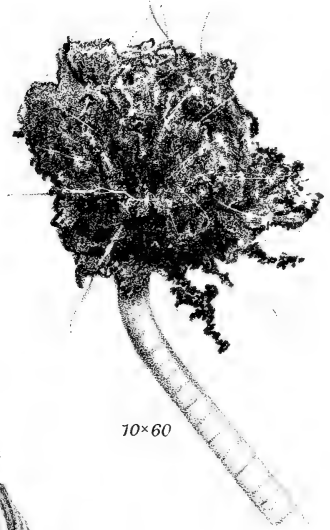
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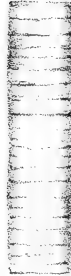
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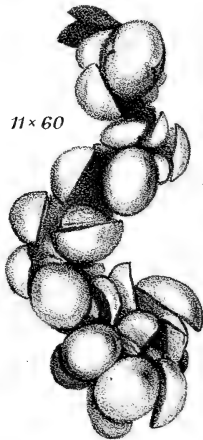
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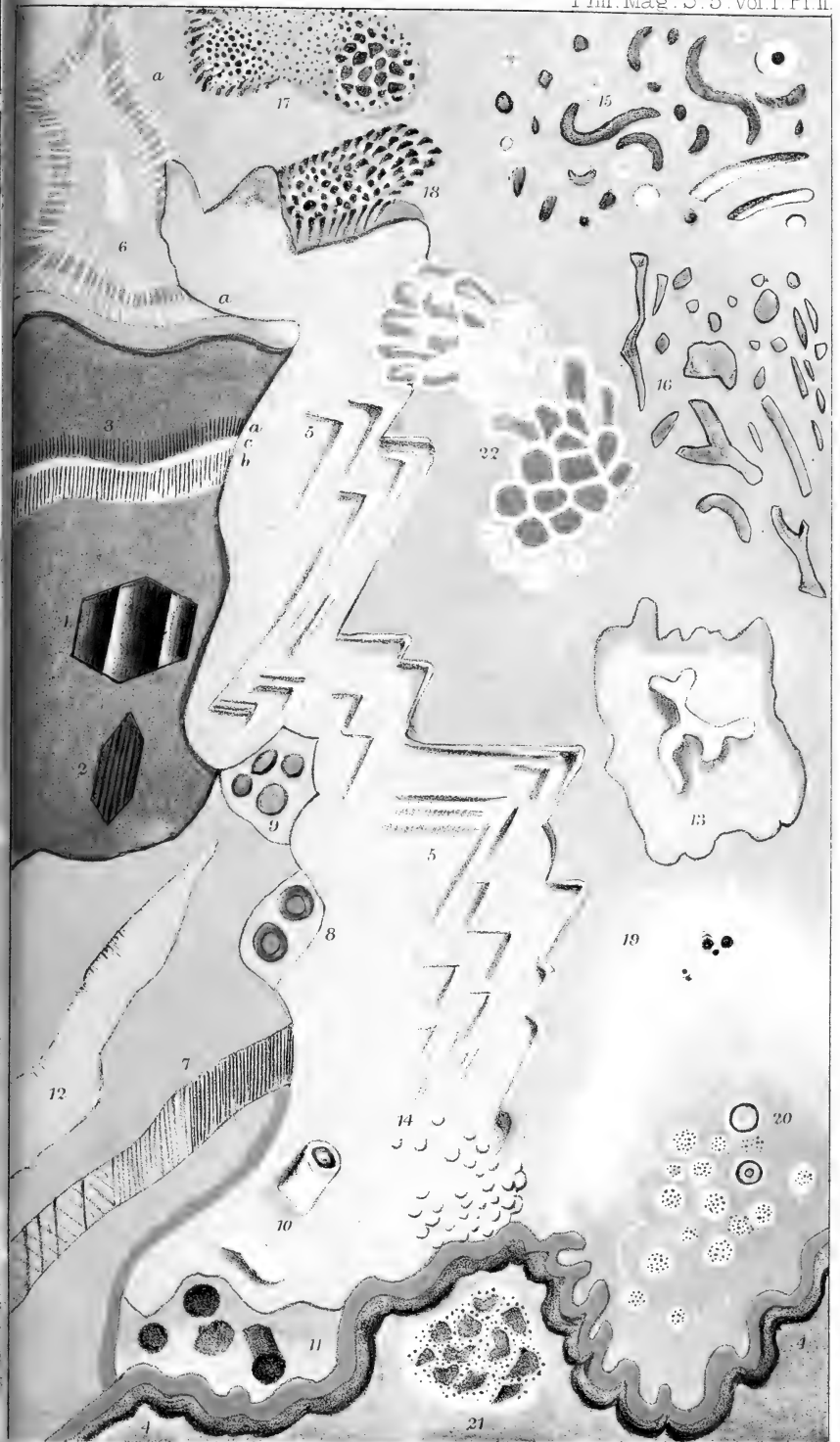


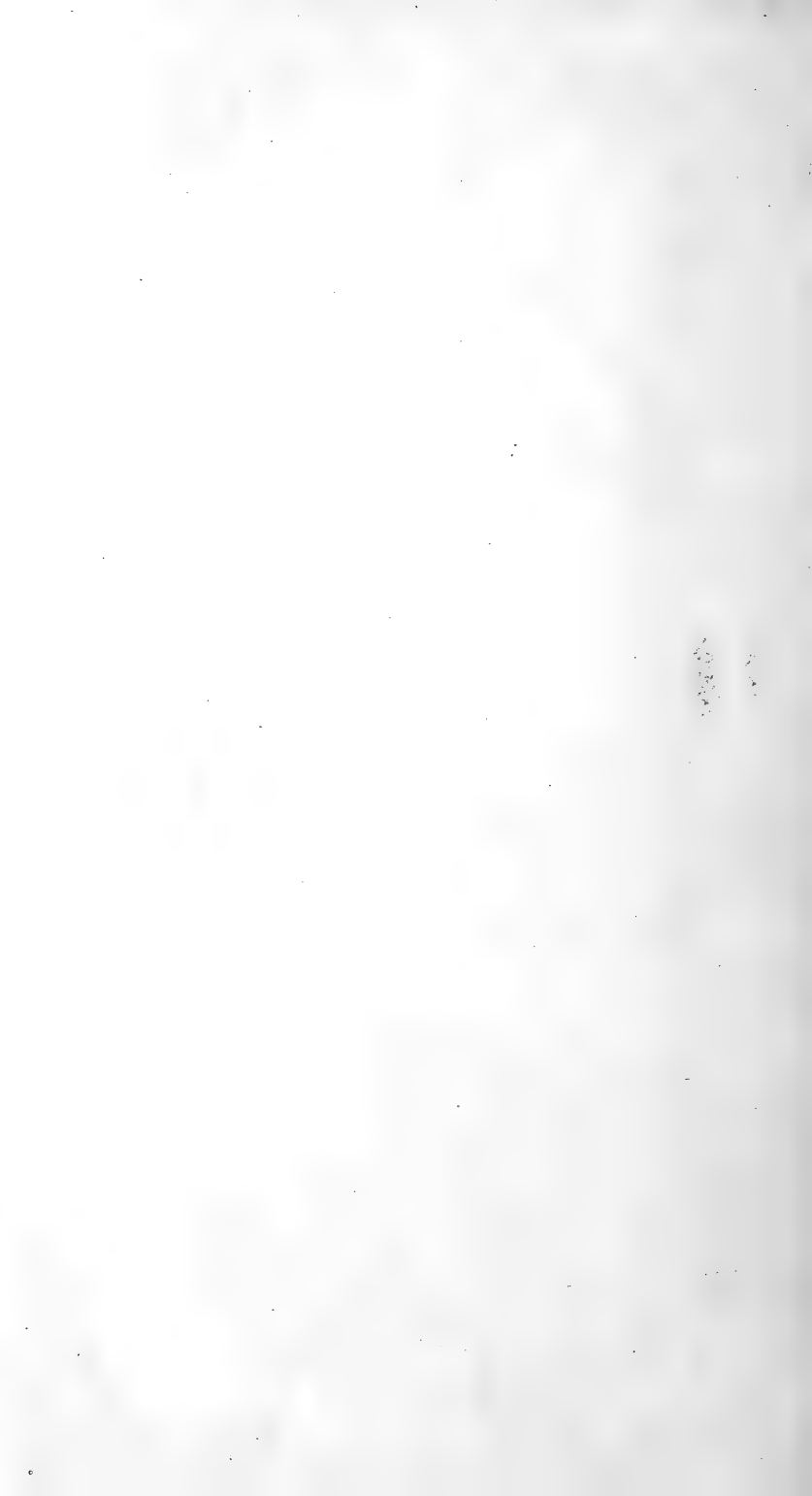
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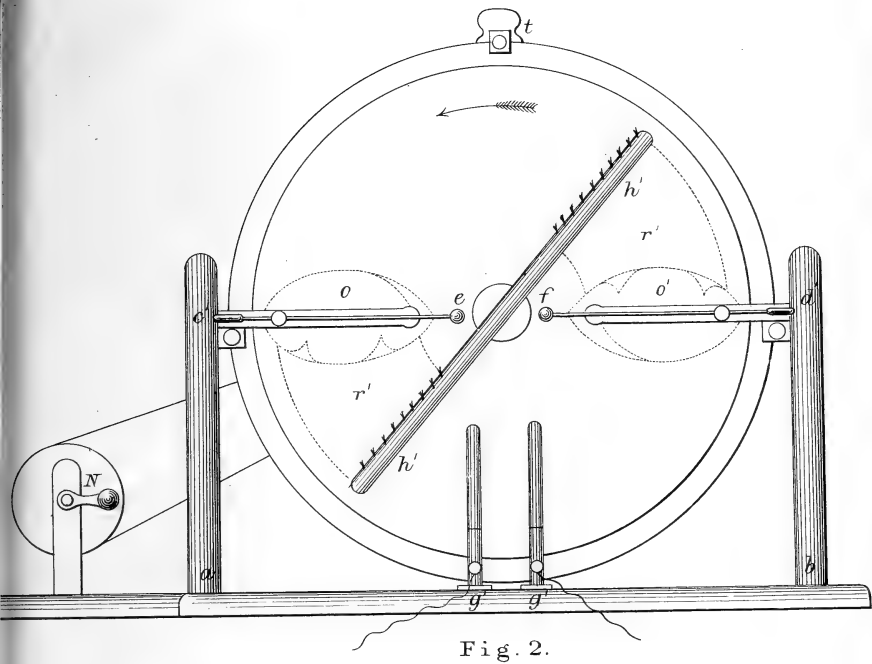


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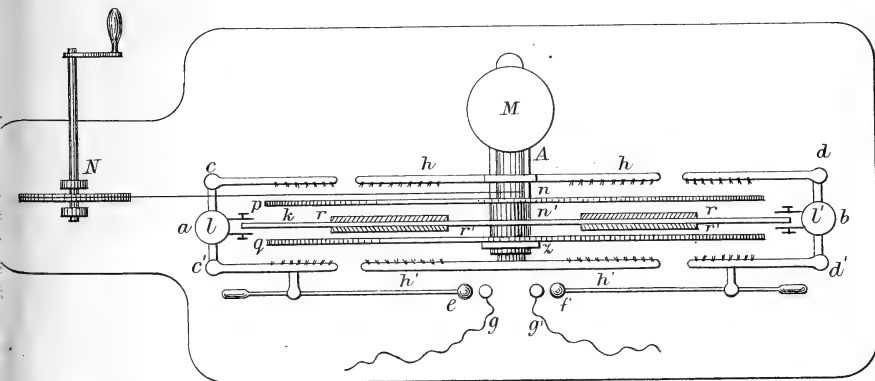


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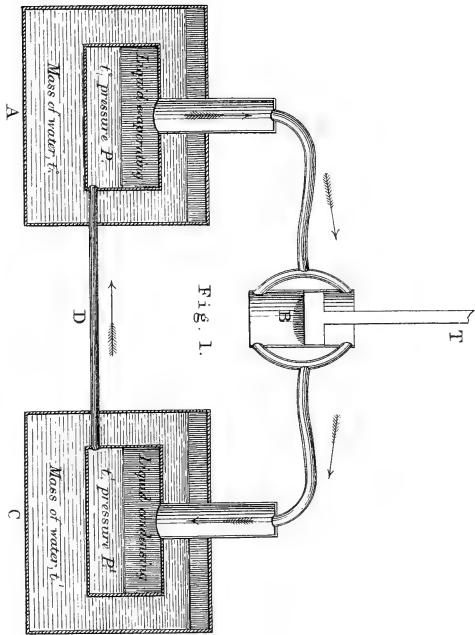
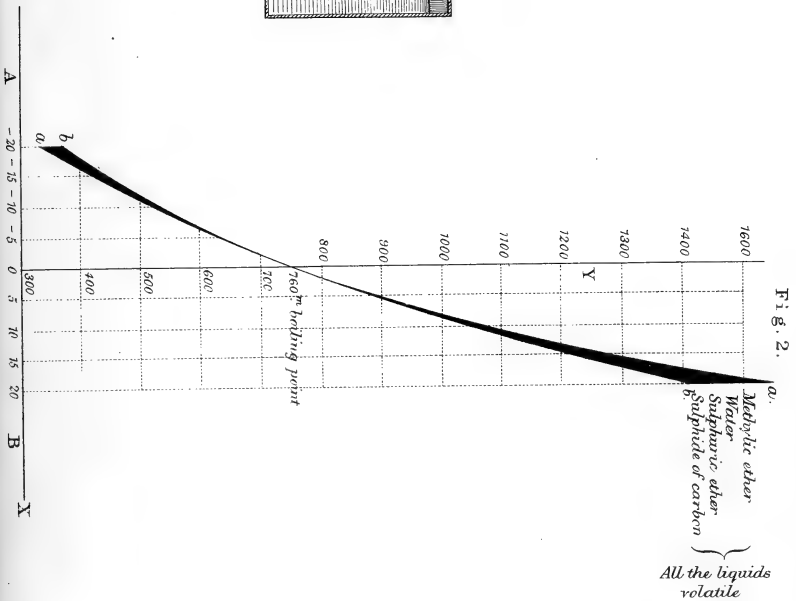
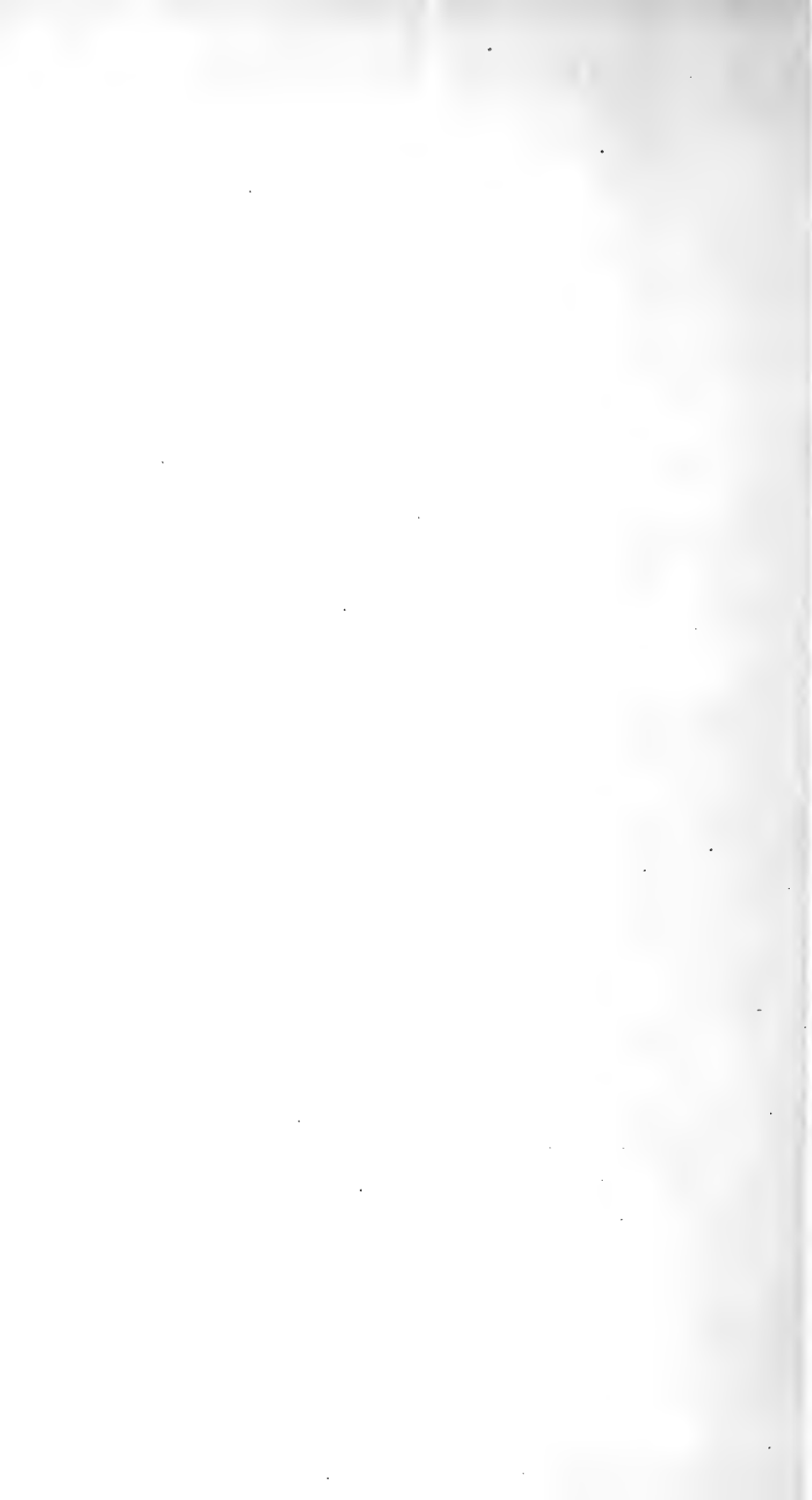


Fig. 1.

In Fig. 2, the right line A B of the abscissa on the temperatures should cut the axis of Y or the pressures at the point O. For the figure we make it pass at the point 300, which makes no change in the curve.





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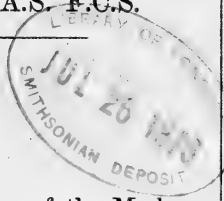
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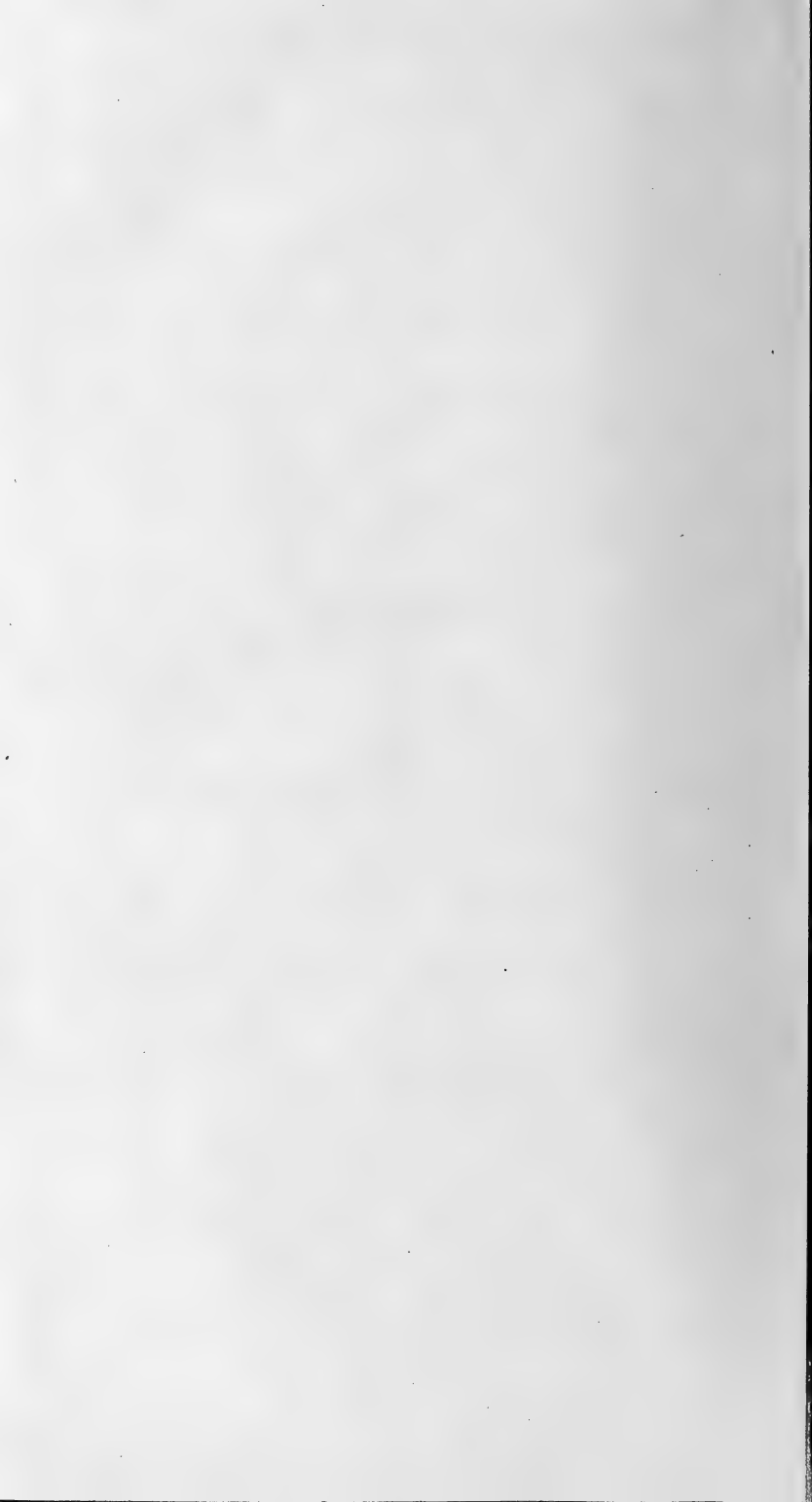
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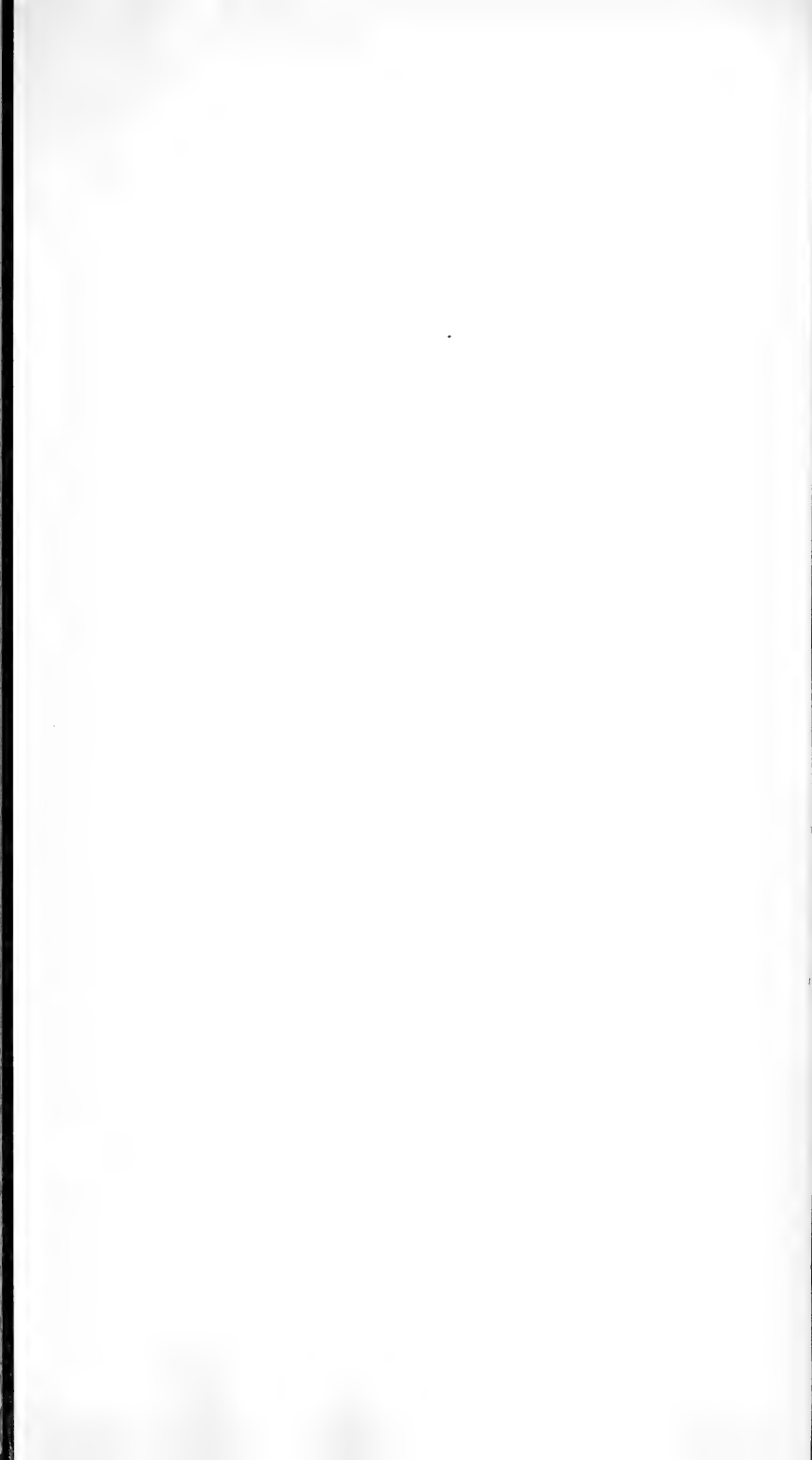
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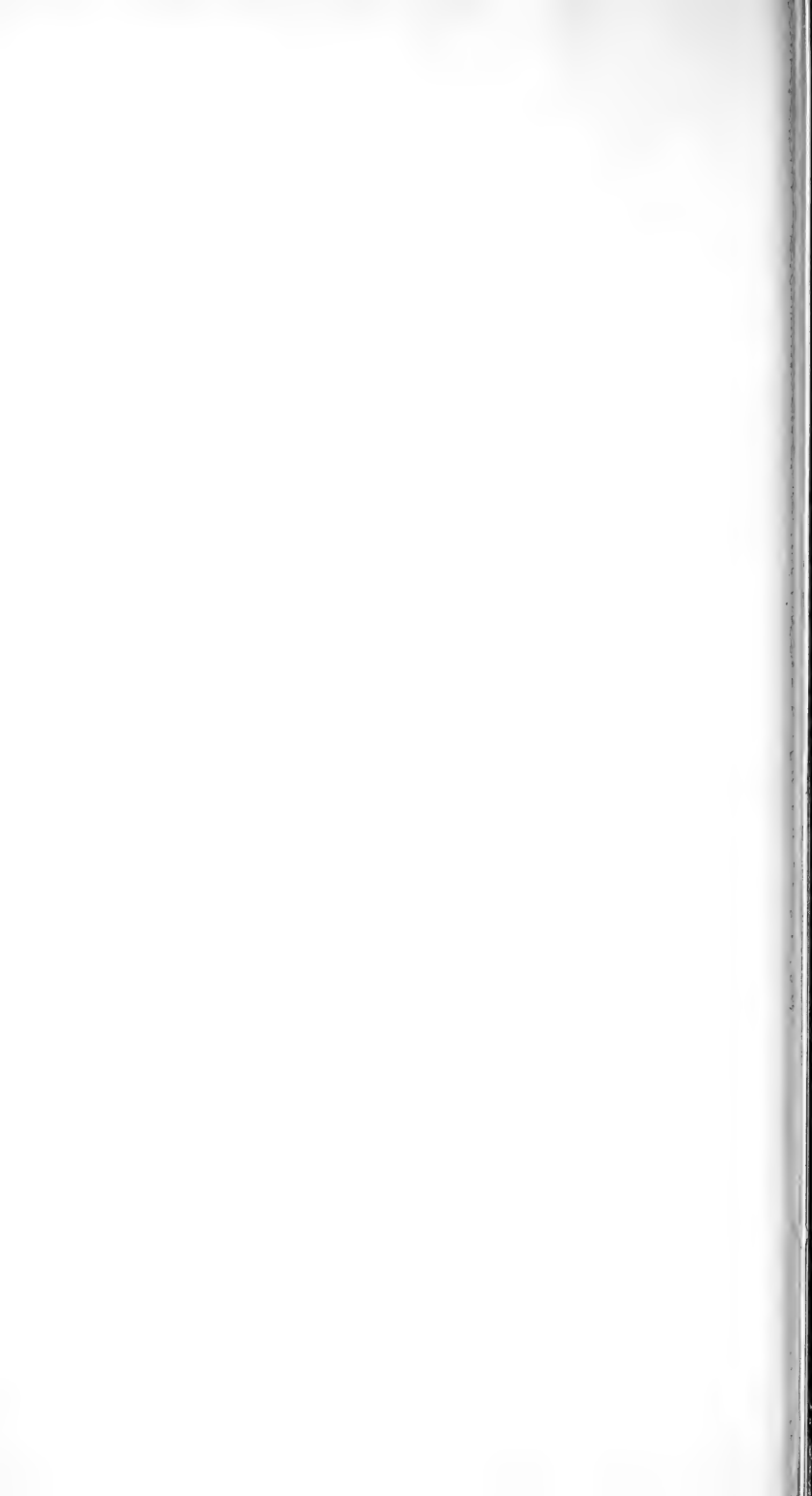
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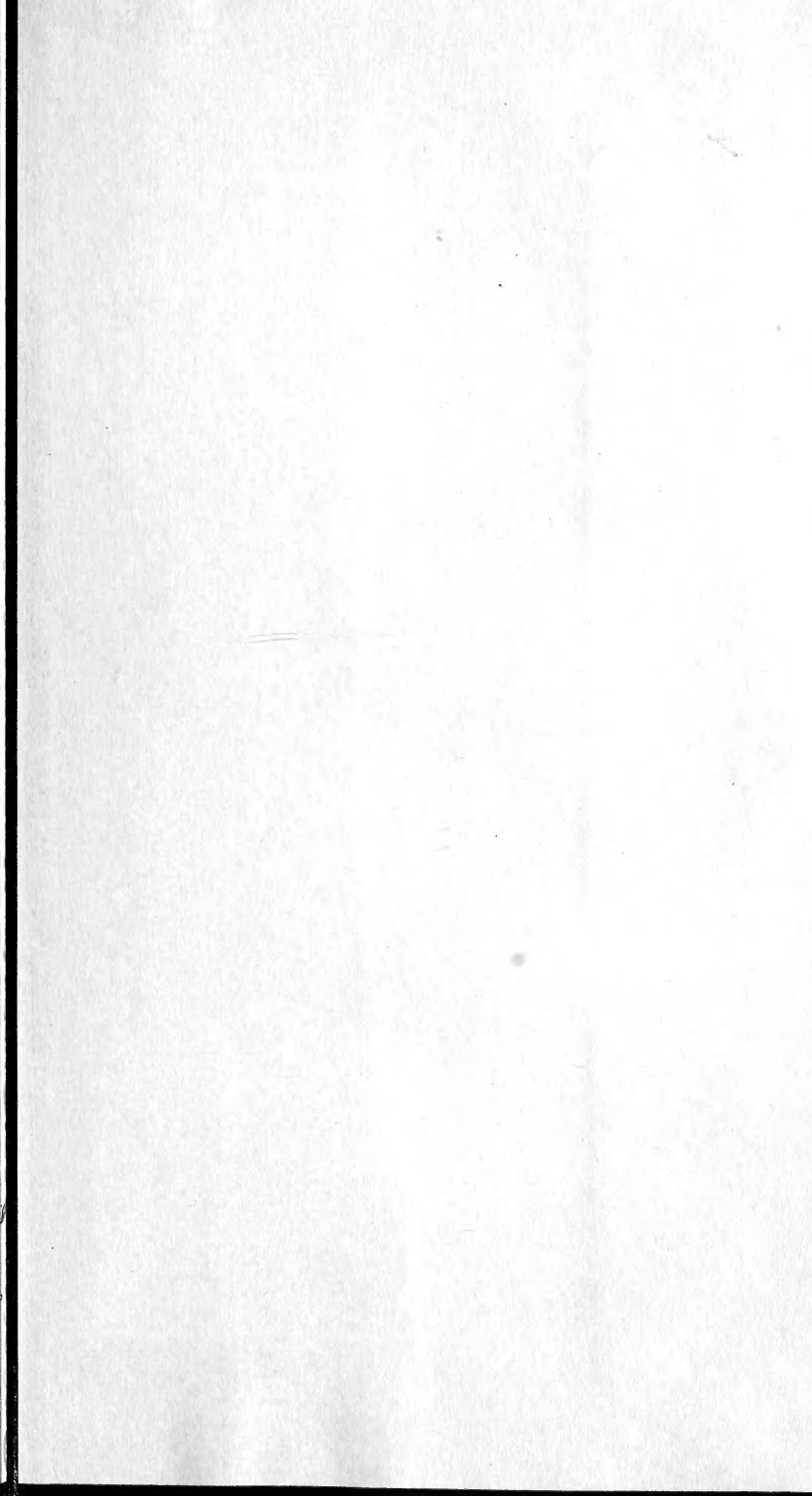
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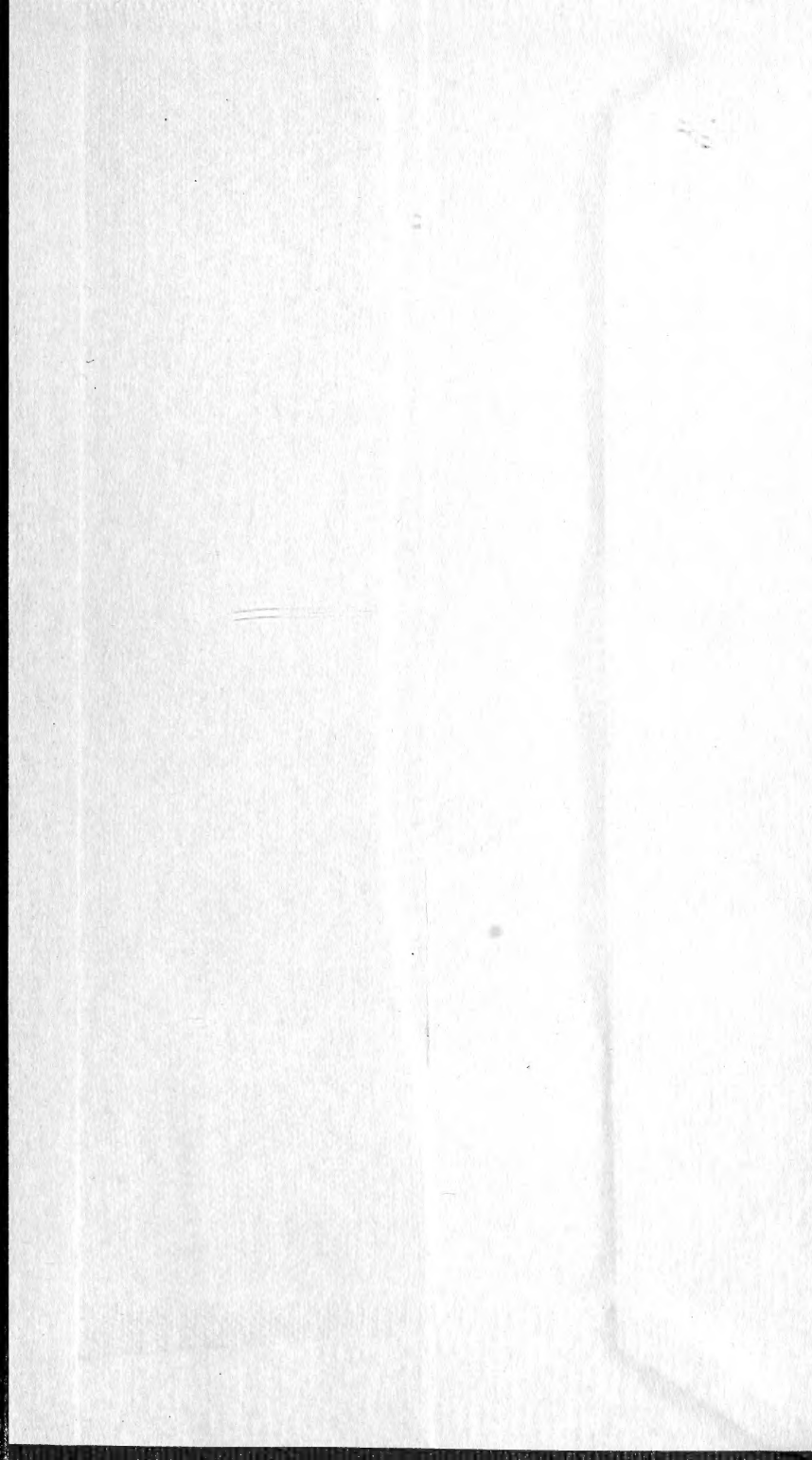
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